THE
QUARTERLY JOURNAL,

January, 1822.

ART. I. *Comparative Analysis of Black and Green Tea.*

THE following experiments, made in the laboratory of the Royal Institution, were chiefly undertaken with a view of ascertaining whether the different effects usually attributed to black and green tea, are referable to any peculiar principle existing in the one which is not to be found in the other. They will also tend to throw some light upon the relative composition of teas of different prices. To ascertain the nature and extent of adulterations of any kind is not our present object: the various specimens submitted to examination were obtained from most respectable sources, and undoubtedly genuine as imported; what processes the tea may be submitted to in China, or what mixtures and additions it may there receive, are curious and interesting matters of inquiry, and deserve further investigation than they have as yet received.

Experiments with Black Tea.

A

One hundred parts of the finest black tea sold in the shops at 12*s.* per lb., were digested in repeated portions of boiling water until it entirely ceased to act upon the residue; the leaves were then dried, and were found to have lost thirty-five per cent. in weight; they retained their original colour. The infusion was evaporated, and yielded a dark brown transparent extract, very astringent, and of a nauseous bitter flavour.

B

The leaves, exhausted of all their matter soluble in water, were digested in alcohol, (sp. gr. 820°), to which they imparted a deep brown colour, and considerable odour of tea. The alcohol being evaporated, yielded a resinous extract of a more agreeable smell and flavour than that obtained by water. The leaves were now colourless, and without the smallest remaining taste; they were dried and had sustained a further loss of twelve per cent.

One hundred parts, therefore, of the finest black tea contain forty-seven parts of soluble matter, thirty-five of which are taken up by water, and twelve by alcohol.

C

A solution of isinglass was carefully added to the aqueous infusion of one hundred grains of the same black tea, as long as it caused a precipitate, which, being dried at a temperature not exceeding 212°, weighed twenty-eight grains.

D

The above experiments were repeated with a sample of the commonest black tea sold at 6*s.* per lb. The weight of the soluble matter imparted to water was precisely similar, nearly 35 grains from 100: but the leaves, having been exhausted by water, only imparted six grains of soluble matter to alcohol. The flavour of the aqueous extract was nearly the same as that of the former two.

E

A variety of samples of black tea were submitted to distillation with water, but the distilled water had acquired in all cases a very slight vegetable flavour only; it contained no appreciable quantity of vegetable matter, and was not obviously different from tea of different degrees of excellence.

Experiments with Green Tea.

A

One hundred parts of fine green tea digested in repeated portions of water, sustained a loss amounting to forty-one parts; the leaves being separated and dried, still retained a greenish brown colour. The infusion, carefully evaporated, afforded a brown transparent extract, highly astringent and bitter, and having a peculiar flavour unlike that of the original tea.

B

The residuary leaves of the last experiment were transferred to alcohol, with which they formed a green tincture; when the whole of their soluble matter was thus withdrawn, they were dried, and were then of a pale straw colour, brittle, and quite insipid. They had sustained a further loss of ten parts.

The alcoholic solution being evaporated to dryness, yielded a highly fragrant olive-coloured resinous extract, scarcely acted upon by cold water, but perfectly redissoluble in alcohol. Its solution diluted with water became turbid, and deposited a pale olive-green precipitate of a slightly bitter flavour, and smelling very strongly of green tea.

One hundred parts, therefore, of the best green tea contain fifty-one parts of soluble matter, forty-one of which, having the properties of tan and extractive, are imparted to water; and ten subsequently abstracted by alcohol, of a resinous nature.

C

An aqueous infusion of one-hundred grains of the same tea was mixed with solution of isinglass; the precipitate, when rendered as dry as possible by a temperature not exceeding 212°, weighed thirty-one grains.

D

A series of similar experiments were made upon a very inferior sample of green tea, sold at 7*s.* per lb. This only imparted to water thirty-six per cent. of soluble matter; but the leaves, subsequently digested in alcohol, lost eleven grains; so that the entire soluble contents of the good and bad tea, are to each other as 51 to 47; but as far as the mere agency of water is concerned as 41 to 36.

E

Green tea was mixed with water and submitted to slow distillation; the liquid which passed over had acquired a little of the fragrancy of the tea, especially of the finer samples, but not the smallest portion of essential oil, or other vegetable matter could be detected in it*.

The above experiments shew that the quantity of astringent matter precipitable by gelatine is somewhat greater in green than in black tea, though the excess is by no means so great as the comparative flavours of the two would lead one to expect. It also appears that the entire quantity of soluble matter is greater in green than in black tea, and that the proportion of extractive matter not precipitable by gelatine is greatest in the latter.

Sulphuric, muriatic, and acetic acids, but especially the first, occasion precipitates in infusions both of black and green tea, which have the properties of combinations of those acids with tan. Both the infusions also yield, as might be expected, abundant black precipitates with solutions of iron; and when mixed with acetate, or more especially with sub-acetate of lead, a bulky buff-coloured matter is separated, leaving the remaining fluid entirely tasteless and colourless. This precipitate was diffused through water, and decomposed by sulphuretted hydrogen; it afforded a solution of tan and extract, but not any trace of any peculiar principle to which certain medical effects of tea, especially of green tea, could be attributed.

One property of strong infusions of tea, belonging equally to black and green, seemed to announce in them the presence of a distinct vegetable principle; namely, that they deposit, as they cool, a brown pulverulent precipitate, which passes through ordinary filters, and can only be collected by deposition and decantation; this precipitate is very slightly soluble in cold water of the temperature of from 50° downwards, but it dissolves with the utmost facility in water of 100° and upwards, forming a pale brown transparent liquid, which furnished abundant precipitate in solutions of isinglass, of sulphate of iron, of muriate of tin, and of acetate of lead; whence it may be inferred to consist of tannin, gallic acid, and extractive matter.

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* "I distilled half-a-pound of the best and most fragrant green tea, with simple water, and drew off an ounce of very odorous and pellucid water, free from oil, and which, on trial, shewed no signs of astringency." LETTSOM's *Natural History of the Tea Tree*, London, 1799, 4to.

Some of Dr. Lettsom's experiments would seem to shew that the noxious effects of tea are referable to the volatile and odorous principle which thus passes off in distillation; and he thinks that those who suffer from them, but yet cannot omit this favourite beverage, might take it with more safety if previously boiled for a few minutes to dissipate the fragrant principle. In all the forms which Du Halde relates for administering tea as a stomachic medicine among the Chinese, it is either ordered to be boiled or otherwise so prepared as to dissipate its fragrancy.

When tea leaves have been exhausted by water repeatedly affused, the above experiments shew that alcohol is still capable of extracting a considerable quantity of difficultly soluble matter; this substance, again infused in boiling water dissolves with difficulty, furnishing a liquid which smells and tastes strongly of tea, and which, were it not for the expense of the solvent and the trouble attending its separation, might perhaps be profitably employed.

Though the above experiments shew that tea contains, upon an average, from 30 to 40 per cent. of matter soluble in boiling water, it is not to be supposed that so large a proportion is taken up in the ordinary process of making tea; on the contrary, from tea leaves in the state in which they are usually thrown away, there is still contained from 10 to 14 per cent. of soluble matter, capable of affording a sufficiently pleasant beverage, though it must be granted that the most agreeable portion of the tea, consisting probably of the purer tannin, or astringent matter, and of the whole of the aroma, is taken up by the first affusion of hot water; and that, subsequently, the bitter and less soluble extractive matter are dissolved, furnishing what is usually called *strong tea*, but infinitely less agreeable than the earlier infusion. Hence it is that the real epicure in this article imitates, in some measure, the Chinese process of infusion; and only drinks the first made tea, using a fresh, but small proportion of the leaves for each successive cup.

The following table shews the respective quantities of soluble matter in water and in alcohol, the weight of the precipitate by isinglass, and the proportion of inert woody fibre in green and black tea of various prices; it is given, not as throwing any important light upon the cause of the different qualities of tea, but as containing the results of actual experiments; it may also perhaps save some trouble to future inquirers.

One Hundred parts of Tea.		Soluble in Water.	Soluble in Alcohol.	Precipitate with gelly.	Inert residue.
Green Hyson	14*s*. per lb.	41.	44.	31.	56.
Ditto	12*s*.	34.	43.	29.	57.
Ditto	10*s*.	36.	43.	26.	57.
Ditto	8*s*.	36.	42.	25.	58.
Ditto	7*s*.	31.	41.	24.	59.
Black Souchong	12*s*.	35.	36.	28.	64.
Ditto	10*s*.	34.	37.	28.	63.
Ditto	8*s*.	37.	35.	28.	63.
Ditto	7*s*.	36.	35.	24.	64.
Ditto	6*s*.	35.	31.	23.	65.

ART. II.—*On the Chart of Shetland.* *By* J. M'CULLOCH, M.D., F.R.S.

HAVING been lately engaged in a geological survey of the Shetland islands, I had occasion to lament the deficiency which British Geography labours under in the want of a map of this district; a want which was productive of considerable labour and much uncertainty, and which eventually rendered it impossible to deduce any satisfactory conclusions respecting the general direction of the strata, or the dependency and connexion of those of a similar nature which were separated by wide intervals. Had even the maritime outline been tolerably correct, these difficulties would have been easily overcome, as the interior country is so little distant from the sea in any part, that a map capable of answering the ends in view could have been constructed with very little labour. The sea-chart of Shetland is, on the contrary, not only grossly incorrect in its general geological details, but, with a very few exceptions, utterly unfit for the purposes of navigation. It would be better, indeed, if no such chart existed; as, except in the very few instances which I shall presently point out, a reliance on it is in danger of tempting a vessel to its destruction. It has not therefore even the negative quality of being useless.

The deficiency of the chart is not so generally known as it ought to be. The name of Captain Preston, which is attached to it, is no less likely to mislead those who are unacquainted with its incorrectness, than the apparent decision with which the rocks, soundings, and anchorage, are laid down; and it is too late to ascertain the position of a rock when a vessel is on it, or the badness of an anchorage when she is embayed on a lee shore. In this respect, indeed, there is a striking constrast between the chart of Orkney and that of Shetland; while the excellence of the former may also have the bad effect of tempting those who have navigated by it, to place the same reliance on the latter; unaware that the survey of Orkney was conducted by M'Kenzie with the greatest care and anxiety, and that the chart of Shetland is little better than a map-seller's compilation, supplying the want of documents with conjectures.

To render these deficiencies better known through the medium of this Journal, is an act of justice which public benefit claims; criticism is not always employed in so pure and good a cause, nor can our defects be remedied till they are pointed out; and it is by no means generally known that this part of British hydrography is in so imperfect a state. If it were known, it is certain that the department of our government which takes charge of these matters, would have long ago found a remedy; as is proved by the laudable anxiety it has always shown to improve the art of navigation. The recent establishment of a light-house on Sumburgh-head is, indeed, an earnest of a desire to render the navigation of Shetland more safe than it has yet been; and it is to be hoped that it will, ere long, be followed by a nautical survey of all these islands*.

The present chart is stated to have been drawn up from the observations of Captain Preston, and the latitudes and longitudes of Admiral Lowenorn. A recent addition has been made to it, of a very accurate survey of Brassa Sound by Captain Ramage, who has also published a correct chart of Balta Sound, which, however, is not appended to the general chart. Some alterations have also been made in the original chart, down to 1820, which are said, in the work, to be "considerable improvements." These alterations are, however, trifling, and relate to little else than some of the latitudes, and to a correction of the longitude of Brassa; a new scale having been fitted on the old plates, and a rock added, conjecturally, to the east of Fetlar, as the cause of the loss of the Hound sloop, of which the place is, nevertheless, at this moment unknown.

It is, however, not possible to discover from what authorities the chart has actually been drawn up. It is understood in Shetland that Captain Preston did not survey the eastern coast, having been drowned while engaged on the west side of the island. It is certain, from internal evidence, that he could not have examined the west side throughout, as the coast from Papa Stour toward the north is exceedingly incorrect. His labours were probably limited to Valley Sound and the Voes as far as Scalloway; although, even these are so incorrectly surveyed, that it is probable the compiler had no access to his documents in a complete state. Neither has the compiler taken advantage of Collins's survey; which, limited as it is, is far more accurate than the present chart is in the same places; as any one may see who will take the trouble of comparing his chart of the two Burras with that which is given, and with the actual form of these islands*.

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* The Commissioners under whom this elegant building has been erected have doubtless proceeded on the best evidence in choosing this place, but the situation is unfortunately too high. In the thick weather so common in Shetland, particularly with easterly winds, this lighthouse must always be involved in mists and fogs; and, even in the ordinary westerly winds, it arrests the flight of the clouds. It is to be feared that the light will frequently be invisible, and particularly when it is most wanted; and that it will thus become an additional source of danger rather than of security. There is unfortunately no lower point at the southern extremity of these islands where it could have been built, so as to serve the purposes of ships coming from the eastward as well as the westward; but many seamen give a preference to Moussa Island, as the chief want is that of a night-mark for vessels intending to make Lerwick. Such a light-house, together with a small light on the Nab, would render that harbour accessible even in the darkest night.

It is not my intention to go through all the details of this chart; nor would it indeed serve any purpose, unless I could have accompanied it by a large copy, which the dimensions of this Journal do not admit. I shall therefore limit myself to a few observations on some of the most remarkable points, for the purpose of establishing the justice of this general censure; those who may be possessed of a copy, may follow these observations without difficulty. The enumeration of even the few particulars which I have selected for remark, will amply justify this general criticism; and it is hoped that it will have the effect which is intended, of exciting the attention of some individual, or that of the government, to the subject; the object being, not to censure error or neglect, but to instigate to a remedy. Having entered almost every harbour under an excellent pilot, I am the more easily enabled to point out where the defects lie, but shall limit myself to the most prominent†.

One of the most important circumstances in the navigation of all the groups of islands which beset the coast of Shetland, is the nature of the tides: their strength, or velocity, their direction, the times of change, their interference, and the currents which are formed among them. Without an accurate knowledge of this nature, the best geographical survey would be utterly useless. If a vessel is not acquainted with the time of change in all these intricate channels, it will often be found impossible to reach the destined harbour, or to effect the intended passage; as the periods of ebb and flood are so materially influenced by the forms of the land in many places, by the collisions of two floods or ebbs from different quarters, or by the interference, perhaps, of the flood of one channel with the ebb of another. Not unfrequently also, it becomes necessary, in shifting from one harbour to another, so to time matters as to secure a portion both of ebb and flood; since, in consequence of the currents, or depths of water in such harbours, or the existence of bars and shoals, the object may as effectually be defeated by taking too large or too little a portion of the one, as by miscalculating the the time required to effect the passage. With a leading wind, or a favourable breeze, it is true, an accurate attention to the tides is often of little consequence; but cases are constantly occurring, in which, if the passage is not effected by the tide, it will not be effected at all; or the vessel may be caught at sea in a dark night, an event which is always a source of great peril on coasts of this nature, or else be embayed on a lee shore subject to any casual change of adverse wind, which, in these regions, often rise with great violence and incredible rapidity. Very often, security is to be obtained by taking shelter in an eddy, or in the still water which is often found to the lee of the current that sets on some island; but it is unnecessary to detail all the cases, too well known to seamen, and particularly to those acquainted with the Scottish islands, in which an accurate knowledge of the tides is as indispensable as that of the coasts and harbours themselves.

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* Although not an advocate for monopoly, the publication of maps, or at least of sea-charts, on the correctness of which so many lives, and so much property depend, ought to be restrained to the hands of government. The temptation arising from a little profit, is too often an inducement to publishers to construct these charts from imperfect and imaginary documents, careless of the consequences which must result from their incorrectness, and, it is to be hoped, not aware of the fatal results which are so often produced by their inaccuracy.

† The accurate and universal knowledge of this pilot, whose name is Peter Anderson, would afford great facility to any one intending to construct a chart of these islands: and it is an opportunity which, once lost,

is not easily replaced; as he is the only person in the islands who is acquainted with every rock and harbour in them. I need not point out to surveyors how much time is saved in investigations of this nature by such a coadjutor.

In this important part of every sea-chart, that of Shetland is lamentably deficient; and it affords a strong contrast with that of Orkney, where all these particulars are recorded with the greatest fidelity and accuracy. There is, in fact, in the chart of Shetland, not the slightest indication even of the direction of the flood, except on the west coast, far from land, and for a small space on the eastern; in both of which it is little better than conjectural, and serves no useful purpose. This most essential circumstance is not only entirely omitted in the channels and near the shores, where a knowledge of it is most wanted, but it is not even noticed on the northern extremity of these islands, where the violence of the tide is such as materially to affect the plan of a vessel attempting that passage, and is extremely dangerous to boats. I shall content myself with enumerating a few of the channels where the tides are most troublesome, or where the defect of this part of the chart is most likely to be felt. In all these cases, it must be presupposed that there is not a leading wind, or a sufficient breeze; and when it is stated that the velocity of these tides often reaches to five or six knots an hour, or even much more, it will easily be understood that such a wind may often be wanting, particularly to deep and slow vessels.

Passing over the navigation of Brassa Sound, since a pilot may always be obtained there, I shall first remark, that neither the anchorage of Simbister Bay in Whalsey, nor that of the Out-skerries, can be taken without an accurate knowledge of the time and nature of the tides; owing to the shallowness and intricacy of the channels leading to them. The same remark may be made on the sound of Uyea and that of Balta; particularly if a vessel attempts to take the latter harbour by the northern passage. In shifting from the former harbour to the latter, it is necessary to make use of the latter part of the flood and the first of the ebb; since, without the first of these, it is difficult to beat out of the harbour of Uyea; and, without the last, equally inconvenient to beat into that of Balta. If a vessel, again, is desirous of going from Balta Sound to Cloup Voe, or the western parts of Yell, or from Uyea harbour to the same places, the chart does not indicate that the pasage round the Scaw in the former case, or that of Blumel Sound in the latter, are inexpedient, or even, in certain circumstances, dangerous; and that it is far preferable to take the southern passage through Yell Sound. The strength of the current through Colgrave Sound, though far less than that of the stream which runs with such velocity through Blumel Sound, renders it also necessary to be well acquainted with the times of ebb and flow through that passage; no less in merely sailing through it, than in attempting to make the harbour of Basta Voe. As, in leaving a harbour, every vessel has an opportunity of ascertaining the state of the tide, it is unnecessary to point out the peculiar situations in any of these islands, in which it is necessary to possess this knowledge for that purpose; but I may remark, generally, that with respect to the making a great proportion of them, a vessel may often fail in its object unless that circumstance is previously well known, and thus be compelled to stand out to sea again.

In passing round the Skaw, or Papel-ness in Yell or Fedaland Point in North Maven, it is equally necessary to be accurately acquainted with the times of change; as the strength of the current is considerable in all these situations, and it is requisite to take advantage of the ebb and flood both, for doubling these headlands; but on this subject the chart gives no information. The whole of the flood or ebb is equally required for making the passage from Fedaland to Hillswick, or through Swarbuck's Min; or from Papa Stour to the southern harbours of Æthsting, or the reverse; and, in all these cases, a miscalculation of the time or velocity of the current, if there is a short wind or a head sea, or both, may be productive of the most serious inconveniences. The same remarks might be extended to all the remainder of the western shore; but it is unnecessary to enter into minute details of cases which have occurred in my own experience, and which must occur to all vessels attempting this navigation without a well-informed pilot.

To a stranger, attempting to make any harbour, it is essentially necessary to have some physical marks, or picturesque appearances, if that term may be used, by which the spot can be recognised under different positions, or under that position, at least, in which it is most likely to be seen. The picturesque representations of coasts and headlands in sea charts are not often accurately or characteristically given; and the necessity that every surveyor should have a facility in drawing landscape, is but too obvious in many better charts than that of Shetland.

A great aid to the judgment is afforded, in all these cases, by the mode of expressing the shores on the geographic outline; whether it be low and sandy, or skirted with low rocks, or consisting of cliffs of different degrees of elevation. In the chart of Shetland these circumstances seem often to have been placed at random; while equal value is often given to rocky shores of a few feet in elevation, and to cliffs reaching to many hundreds. To quote examples is but too easy. The west and east sides of Foula are laid down as if they were of the same elevation; whereas, on the former side, the cliffs exceed a thousand feet in height, and the latter is almost uniformly low, and, in some places, indeed, quite level with the sea. The same error is found in many parts of Yell, Unst, and Fetlar; where low shores, and even sandy bays, are laid down as if they consisted of lofty cliffs. Trestra Bay in Fetlar, and Uyea Sound in Unst, are remarkable examples of this error. The island of Balta is another; in which the high cliffs of the eastern shore, and the low and often sandy outline of the western, are represented by the same hieroglyphic. I need not enumerate other striking instances of the same inaccuracy; as, to go over them in detail, would be to give an analysis of the whole lengthened outline of this intricate and indented coast.

To the mere geographer, the most gross inaccuracy of the chart of Shetland, consists, not only in the displacement, but in the absolute omission, of many of the smaller islands; some of which are far from being of trifling dimensions. I shall content myself, as before, in pointing out some of the most remarkable of these; as the want of an accompanying chart on a large scale would render the detail of trifling particulars as unintelligible as it would be tiresome. It will be easily understood, that, independently of the mere geographical defects, these errors are of important consequence in this chart, as far as it is intended for a guide to navigators; not only tending to mislead them respecting their position with regard to any given point of the coast, but further endangering their safety, in thick weather, by the unexpected occurrence of land where they expected to find sea.

An island which lies off Scant Ness is entirely omitted; and this is the more unpardonable, as it is the southernmost point of all Shetland, and therefore sufficiently remarkable. Two small islands to the north of Rovie Head, near Grumister, have also been forgotten. The same occurs at the entrance of Catfirth Voe; where one of the two Glitness Islands is left out of the chart. The How Stack, near them, which is a green island, is also laid down as a naked rock. In Oure Voe there is also an island omitted; and another has been forgotten near Hog Island, not far off from its entrance. At the north end of Whalsey are two islands, only one of which is noticed; but the confusion of small islands on the eastern coast of this spot is utterly unaccountable. There are four islands where only one is marked: three of them called the Holms of Ibister, and another, detached, of which I have lost the name. The Rumble and the Grief Skerry are also utterly misplaced; the former being a mile or more out of its true position, to the southward, and the latter appearing to have been transposed from the north to the south side of East Linga, over a space of more than three miles.

The Out Skerries are represented in nearly as incorrect a manner; the three larger islands being either displaced or omitted, so as to produce the most inextricable confusion. In consequence of this confusion, it would be quite impossible for any vessel to recognise these islands, or to attempt to make the anchorage. The geologist is equally puzzled in attempting to reconcile the physical geography of the stratification which he is examining, with the political geography which the map-maker has thought fit to assign to the remote tenants of this melancholy and stormy spot.

To compensate for the loss of an island in one quarter, the same artist has conferred on Unst one which has no existence;

namely, that of Houna, near Norwick Bay; treating as a rock the real Houna, which lies at the entrance of Balta Sound. It would be equally difficult to account for the assigned place of Hascosea, an island more than two miles in length, lying in a much frequented passage, and forming the very important breakwater to Basta Voe; it is moved more than a mile to the southward, so as no longer to perform that office.

In Yell Sound, the proportions, or relative positions of nearly all the islands are entirely perverted, so as to render it difficult, in navigating them, to know which is meant by any one of those laid down in the chart. This misplacement is most remarkable in the Brother Holm; while one of considerable size to the northward, called Little Holm, is omitted. Similar irregularities occur in the position of the Ramna Stacks, and in that of Greenholm, off Fedaland Point, while one of the former is also omitted. As the passage round this Point of North Maven, which forms the northernmost point of the mainland, lies between Greenholm and the land, this error is the more inconvenient and censurable.

To pass over errors of less consequence in the position of Little Wya and Ossa Skerry, in the dimensions and position of that Linga which lies at the mouth of Olna Firth Voe, and in the omission of a small island at the end of Selie Ness, I may remark, that in Wisdale Voe, an island containing not less than a square mile, is converted into a rock. The position assigned to the rocks and islands which crowd the bay, intercepted between Skelda Ness and Burra, are also incorrect; but the inconvenience that might arise from this is, in a great measure, obviated by the channels which lead into Scalloway having been laid down. It is for this part of the chart that the compiler appears to have been indebted to Captain Preston's survey; and it is certainly the least exceptionable part of the work; though I must in justice remark, that an appended chart of Valley Sound and Grueting Voe, on a larger scale, is also very correctly laid down.

Where such errors exist in the positions of islands of such dimensions, and where so many are omitted, it cannot be expected that there is more accuracy in the places of rocks, whether visible or sunk, in which so many parts of the coasts of Shetland abound. It is not too much to say, that, with very trifling exceptions, the whole of them are incorrect; either by reason of omissions, misplacement, or characters wrongly expressed. It will suffice here to mention a very few of the most remarkable cases, as some of them will again come under notice in pointing out the errors in laying down the harbours. How important it is to be accurate in this part of every nautical chart, it is quite unnecessary to remark. If there is one circumstance more than another which is a source of perpetual anxiety and distrust to a vessel attempting to make a harbour, or navigate a channel, it is a doubt respecting the place, existence, and character of rocks; and, more particularly, of those which are not always visible. It does not, indeed, fall to the lot of many mariners to experience the anxiety that is felt by him whose fate it is to navigate coasts of this nature; but those whose business it is to be often engaged among islands and channels like these, know well the hourly risk to which they are subject, where the space of one day alone exposes them to greater hazard than could be crowded into a whole life spent in navigating the ocean. It is not too much to say, that the circumnavigation of the Shetland Islands is attended with more hazard than that of the globe. A correct chart would reduce that hazard to little or nothing; yet that is not only wanting, but the seaman is exposed to the additional risk which must ever arise from the necessity of placing confidence in one which is grossly erroneous.

A number of sunk rocks are marked in this chart as lying near to the Noull of Eswick; whereas there is deep water here close in shore, with a safe passage for ships of any draught between the How Stack and the land.

Respecting the very intricate and dangerous navigation between Whalsey and the main land, or that on the east side of this island, it is only necessary to say, that not a single rock is rightly indicated; the whole being such a scene of confusion in the chart, that it is vain to attempt to specify the errors in words. That which was remarked respecting the islands at the Out Skerries, may also be observed of the rocks, which are equally incorrect in their positions.

In entering Oure Voe, it is not indicated that there is a rocky shoal between Ballasetter Holm and the southern shore, respecting which I had the disagreeable experience of having struck on it in nine feet water.

A rock is laid down off Fetlar, on which the Hound sloop, as formerly remarked, was said to have been lost in 1800. Now the true place of this rock is not ascertained, important as it is; nor did our pilot believe that the place indicated was at all near to that in which that vessel was wrecked; certainly, at least, none of the fishermen, who are perpetually on this coast, were acquainted with the spot. Hence no vessel can approach Fetlar from the east with any confidence, and those steering north or south must either keep a very wide offing, or sail close in shore.

There is an utter confusion respecting the rocks which lie to the southward of Yell, of which the knowledge is exceedingly important, as they lie in the way of vessels intending to pass from the north through Yell Sound. As to the Ramna Stacks, which were already noticed in mentioning the misplacement of islands, the omissions are of less consequence, as the principal are sufficiently conspicuous to form marks for themselves.

There is a passage between Papa Stour and the main land, which would often be very convenient, as enabling ships to save a tide in going for St. Magnus's Bay; but it is so ill laid down that no vessel can venture to take it. Not to prolong a part of the subject, however, on which it would be useless to insist further, I shall lastly remark, that there are great doubts in the minds of the pilots and fishermen, respecting the true place of the Have de Grind rocks, which lie to the eastward of Foula. Such a want of confidence, indeed, was felt by our pilot, experienced as he was, that he declined taking charge of the vessel, and those who have been in similar situations may judge of the uneasiness experienced in standing off and on, in a heavy sea, during a whole night on this coast, with a view of making Foula in the morning.

The profusion of excellent harbours in Shetland is such, that every seaman who has experienced the want of similar refuge in the channels of England, is inclined to express a childish regret at the waste of a commodity, as he is inclined to view it, which, if properly distributed, might almost supply the whole of Europe with anchorages. Even in this profusion, however, is the bounty of nature shewn, as, without the refuge which they afford at almost every point, it would be impossible, at least in the short days of winter, to navigate these coasts at all. It is not possible for those who have not experienced this kind of navigation, to conceive the anxiety which the coming on of darkness or thick weather produces in such intricate channels, amid rocks and on lee-shores, and among currents and tides, which prevent the pilot from forming any estimate of the vessel's place. In such cases even all the harbours of Shetland are not too many; and yet of these there is a large proportion in which the compiler of the chart has placed no anchor, while in others he has marked stocked anchorages, where no vessel would even venture to stop a tide, unless in fine and summer weather. I shall enumerate these errors and omissions in somewhat greater detail, as it is a species of information which can more advantageously be communicated by mere words, than that which was attempted in most of the preceding remarks.

The anchorage at West Voe, near Sumburgh Head, may often be very convenient with an east wind, or with a wind from the west, when Quendal Bay would be too open. The ground is clean and good, and there is no difficulty in beating out, unless the wind were to shift to the southward. It is far more spacious than is represented in the chart, owing to the western promontory having been laid down of more than double its actual breadth, and from the omission of the island off Scant Ness.

The harbours immediately north of Sumburgh Head, namely, East Voe and the Pool, are both exposed and shoal, nor are they safe, even for the smallest class of fishing vessels, although in the chart a stocked anchor is laid down in each. Levenwick, Sandiwick, and Æth's Voe, are almost equally bad; but any incorrectness in these is of less moment, as no vessel would incline to stop in them when equally able to reach Brassa Sound. On the subject of that sound, the minute survey of Captain Ramage, lately appended to the general chart, leaves nothing to be desired. There is nothing to object to the several anchorages laid down in Catfirth, Wadbester, Laxfirth, and Dale's Voes, nor to those in Oune Voe, since an anchor may be let go in almost any part of these inlets.

But in Whalsey the chart of the harbour is so incorrect, that the real anchorage could not be discovered by it without the aid of a pilot; so that in this respect the chart is, to say the least of it, useless. If the anchorage at the Out Skerries, which may often be very convenient, had been properly laid down, there would have been no difficulty whatever in taking it, by attending to the tides. For want of any direction respecting these, from the absence of marked soundings, and from the extremely incorrect position given to these islands and the intermediate channels, no vessel would now dare to enter them; although, so far from the harbour being fit for small vessels only, as the chart says, ships drawing twelve feet water and upwards may lie in it with the greatest safety, and may quit it with any wind, as there are two entrances.

The entrance to Vidlon Voe is perfectly simple; and here the chart has very properly laid down anchors, as it has, with much less propriety, in Burra Voe, at the southern point of Yell, since that harbour is superseded by the much better and neighbouring one of Hamna Voe, although in neither have any soundings been laid down. From this part of Yell to Refirth Voe there is no anchorage, and, although there is fortunately no difficulty in entering this harbour, excepting that arising from its narrowness, it is extremely ill delineated in the chart, nor are any soundings placed in it, so that it cannot be entered without the lead. Basta Voe forms one of the finest harbours in Shetland, or perhaps in the world; but no indication of its nature is given in the chart, and, as usual, it is deficient in the essential circumstance of soundings; so that in thick weather, in which it was my fate to enter it, it is necessary to keep the lead always going. In speaking formerly of the incorrectness of the islands, I remarked that this harbour was principally formed by the island of Hascosea, which covers it, while, according to the chart, no vessel would venture to run for it in a south-east wind, to which, according to the draught, it appears to be open.

Fetlar contains no harbour, yet, for want of soundings in the chart, vessels might be inclined to take Trestra Bay, where the shoal water of this very bad place ought to have been noticed. Uyea Sound is properly marked as an anchorage, but the anchor is laid down too near to the shore of Unst.

Captain Ramage's chart of the important harbour of Balta Sound has supplied the deficiency of the general chart in this place, but a reduced copy ought to have been added to it, as it is impossible otherwise to venture on this harbour, particularly by the northern passage, where the relative position of Balta and Unst is extremely erroneous. It has been long enough before the public to leave the proprietors of the Shetland chart no excuse for not having appended it to the editions sold in 1820.

There is no other harbour in Unst, and although an anchor is laid down in Cloup Voe, at the north end of Yell, it is not frequented. The difficulty of beating out of Whalfirth Voe, against the western swell, also renders that an inconvenient harbour, a circumstance of which notice ought to have been taken in the chart. To the southward of this, near Sandiwick, are two harbours where vessels may lie securely, but in neither of these is an anchor marked.

It is scarcely necessary to notice the omission of anchorage marks in the voes to the south of Waterholm, as these harbours are not wanted in a channel where so many others are at hand; but it ought to have been remarked that there is a very good and a very convenient anchorage in Urha Voe, for vessels which are either employed in Yell Sound, or have not the good fortune of wind or tide to effect their passage through it. This omission is particularly censurable, as the depth of Hagraster Voe makes it inconvenient, and as there is a scarcity of good harbours, compared with the necessity for them, which may often be felt in this channel, on the east side of North Maven.

In Hagraster Voe no anchor at all is laid down, although, for a space of near eight miles, vessels may anchor almost any where in this very secure and quiet place, in clean and good ground.

Colifirth Voe also forms a very convenient harbour for vessels losing the ebb tide in the channel, yet no anchor is laid in it. The draught of this harbour is indeed so incorrect, that no vessel would be aware of its containing a smaller bay within, where there is always smooth water, and excellent soft holding ground.

No anchorage is marked in Burra Voe, to the northward of this, or, as it is sometimes called in the country, North Ru. Yet it is peculiarly convenient for vessels intending to make the passage round North Maven, as, by taking the end of the ebb from it, they may ensure their passage round Fedaland Point, with a whole flood, to reach any of the harbours in St. Magnus's Bay. Independently of this, it is laid down in such a manner that no vessel would expect to find a harbour in it, while a sunk rock in the entrance, which is not easily seen in the smooth and dark water of a bay, receiving so much fresh water from the hills, is entirely omitted. I may as well add here, that which more properly belongs to the examination of the coast outline, that there is here laid down in the chart a bay called Husater Voe, which has no existence, the whole coast from Colifirth Voe to Bura Voe being nearly straight, instead of being deeply indented.

Although an anchorage is laid down in Sand Voe, on the west side of North Maven, no vessel can possibly take this harbour, unless under extreme distress. From its narrow entrance it is utterly impossible to beat out of it, and, from the prevalence of westerly winds, and the almost unceasing heavy swell from that quarter, a vessel once at anchor here might be detained for months. The same reasoning applies, and almost in the same degree, to Ronas Voe, although the entrance is much wider. The difficulty of getting out of these harbours is much increased, and indeed often rendered extremely formidable, by the squalls which blow from the surrounding high land, and by which a vessel is so often baffled in her attempts to stay, while the want of room to wear renders the missing of stays a very dangerous accident, as I have more than once experienced. To avoid future repetition, I may here remark, that Ronas Voe has a deep inlet nearly at right angles to the entrance, which, in the chart, is entirely omitted. Of Hamna Voe, on this shore, where an anchor is also laid down, it is only necessary to observe that, owing to its breadth, it might be safer for the purpose of stopping a tide in, with a wind from the eastward, but that in a westerly wind it affords no shelter whatever.

Although an anchor is laid down at Hillswick, no vessel, looking at the draught on the chart, would attempt to take it, as the line of the coast is made nearly straight where there is a considerable bay. Thus, as happened to myself on getting into this bay, a seaman is tempted to run for Hamer's Voe; a most secure harbour it is true, but out of which it is scarcely possible to beat against a strong west wind, from the narrowness of the entrance, while that attempt is also attended with a hazard which nearly cost the loss of the vessel in which I sailed. I ought here also to remark, that there is an inlet from Hillswick northwards of two miles in depth, which is forgotten in the chart. This, however, is a mere question of terrestrial geography, as the water is too shoal for vessels. As ships may anchor by the lead almost any where to the west of Muckle Roo, the chart is here sufficiently correct, as are the directions to enter by Swarbucks Min. But there are two serious deficiencies in the sailing directions for St. Magnus's Bay, as well as in the chart of that place, which require notice, more particularly as, for want of proper information, a Russian vessel was lost here not long ago, when she might probably have escaped without much difficulty. The tallow, which formed part of the cargo, is still picked up on the shores, being mistaken by the natives for spermaceti, and having undergone some changes from the action of the sea water, which are not uninteresting to chemists. Vessels caught in St. Magnus's Bay with a westerly wind, are directed to run for Swarbucks Min. Now, if the wind is to the southward of west, a vessel, in attempting to weather Muckle Roo, may fail in this object, and become so deeply embayed as inevitably to go on shore on Eglisha, the Longhead, or Isle-burgh-ness, as that above-mentioned did. The direction in this case should have been, to make for Hillswick or Hamer's Voe, as more to leeward, and where the entrance is attended with no difficulty. If once to leeward of the Longhead, it would be too late, with such a sea as the westerly swell sets in here in a gale of wind, to attempt Hillswick, and equally impossible to weather Muckle Roo. It ought also to have been noticed in the chart, that there is no water, except for boats, in Rose Sound, which is, on the contrary, laid down as a wide channel, and without soundings, since a vessel, despairing of weathering the land, might make for this opening, where she would infallibly be lost.

In Papa Stour no anchor is laid down in House Voe, which is not only a good anchorage for ships of moderate burden, but is absolutely necessary for the purpose of waiting the tide to the southward, supposing a vessel to have left the harbours within Swarbucks Min with the ebb. Two or three anchors, on the contrary, are placed in harbours at the north of Walls, where no vessel enters.

The separate survey of Valley Sound and Grueting Voe are correct, although, in a geographical view, the latter inlet is very improperly contracted in its dimensions. It ought also to have been noticed, that no vessel can beat out of the east sound with a swell from the south-west, and that the attempt is attended with the greatest hazard, from the narrowness of the channel, and from the height of the land, which produces baffling squalls, and from there being no room to wear in the passage, nor ground to let go an anchor if a vessel should miss stays. Of the perilous nature of this attempt I can also speak from experience. There is a stocked anchor laid down in Selie Voe which ought not to be there, as it is almost an open harbour. In Frixeter

Voe one anchor is placed; the whole is, however, one immense harbour, capable of containing all the fleets of Europe, but the chart has neglected to notice the shoal water at the entrance, which renders it nearly as impracticable as it is useless on this coast. In Wisdale Voe no anchor is marked, although there is an excellent anchorage near Sand, with a clean channel. Of both these latter bays I may further remark, that the outline is very incorrect; in Wisdale Voe, in particular, a deep bay being gratuitously placed where the shore forms a straight line. It is from errors of this nature that the opinion was formerly given, that Captain Preston's documents could not have been applied to the construction of even this part of the Shetland chart, as such mistakes could not possibly have existed in any real survey, had it even been executed by the most ordinary fisherman.

No anchorage being marked in Stromness Voe, it is almost superfluous to say that the entrance is too narrow, as well as too shoal, for any vessel; but a singular omission of a geographical nature is here deserving of notice. This is the total omission of the prolongation of this voe, which reaches for nearly three miles into the interior country, being connected with the more open bay by a narrow channel, over which there is a bridge. As a part of the sea outline, this should have been inserted, although other inland lakes and objects were neglected, as appertaining rather to the terrestrial geography. The harbour of Scalloway is sufficiently well laid down, but there is a very singular mistake in representing the two lakes of Tingwall as one, and in connecting them with the sea, as if they were salt lakes, whereas the southernmost is separated from it for more than a mile by a tract of meadow-land, which certainly has not been formed since the chart was constructed. Anchors are laid down in several parts of Cliff Sound, where no seaman would think of anchoring, owing to the depth of water, and the squalls from the high land, and where, in fact, there is no occasion for any vessel to stay. A similar remark may be made on an anchorage marked between the two Burras, which can only be required by the smallest class of fishing vessels, and is safe for no other, while the extremely incorrect way in which these two islands are drawn, renders it nearly impossible for any stranger to conjecture where the channels are, or where his position is.

Near St. Ninian's Isle are laid down three anchors, and it would be difficult to say which of these places is the worst harbour, if indeed they at all deserve the name of harbours. In an east-wind, a vessel might stop a tide under St. Ninian's; but, with a wind from the west, no seaman in his senses would make such an attempt. I may also here observe, that the bar which connects St. Ninian's Island with the main land, is laid down in a wrong place; being nearer to the middle of the island.

The last harbour in the circuit is Quendal Bay. Here Collins's chart is more correct than that which is here reviewed; although deficient with respect to the point that covers the proper anchorage under Quendal house. This is a wide and excellent bay, even in winds from the west; nor would there be any difficulty in getting to sea, should the wind shift to the southward, unless there was a very heavy swell; as it is sufficiently wide to enable a vessel to beat out without difficulty.

It would be an endless task to go over the subject of soundings, and I shall therefore be content with remarking, in a general way, their incorrectness and deficiency. Where these were of most importance, they have already been noticed as far as is necessary for the purposes of this criticism, in the preceding remarks on the harbours.

Neither is it necessary to make any detailed remarks on the very few sailing directions which are appended to the chart; that which relates to St. Magnus's Bay having already been examined. I shall only observe in addition, that the passage through Cliff Sound, either into, or out of, Scalloway, is always inexpedient, owing to the squally nature of this bay and narrow channel, and the nicety required in passing the bar of Trondray, on which there are only twelve feet at high water.

With respect to the general outline of Shetland as given in the chart, I must limit myself to a few cursory remarks on some of the most prominent errors which are to be seen by the eye alone; as I had no opportunity of making any accurate observations by the aid of instruments. Nor indeed can any correct notion of the heap of errors which it displays, be given in words. In a nautical view, this species of error is certainly of far less importance than those already pointed out; yet it is not the less discreditable, that so large a portion of the British dominions, remote as it may be, should have remained so long neglected as it is, but as it cannot now continue much longer. The interior geography is, in fact, not merely little better, but somewhat worse than a blank; as that which is given is incorrect, and as the record of any one object only tends to mislead, by causing the traveller to suppose, that where nothing is laid down, nothing is therefore present. Near Sumburgh Head, it is not indicated that the connexion between this promontory and the main land is so slight that it is almost insulated by a tract of loose sand scarcely higher than the level of the sea; nor would the near approach of the voes on the east side to Quendal Bay, be supposed, from the outline in the chart. There is no incorrectness of much moment, as far as the objects of the chart are concerned, from this point to the Naull of Eswick, where a small but deep inlet is entirely omitted. I may remark indeed, once for all, to avoid tedious repetitions, that throughout the chart in general, the voes are rarely carried to a sufficient depth within the land, and are often equally contracted in their lateral dimensions.

The incorrectness of the outlines of Whalsey Island, and of the Out Skerries, were already mentioned in speaking of the anchorage in these places; but it is fair to remark, in extenuation of these and other errors, that Oure Voe and Vidlon Voe are, on the whole, very well delineated. In Yell there is a deep bay at Quyon which is very slightly marked in the chart; but as it is not used as an anchorage, on account of some rocks at the entrance, the error is of the less moment. The extraordinary incorrectness of the coast and the harbours immediately to the northward of this, was already mentioned in speaking of these harbours, and of the misplacement of Hascosea Island.

The outline of Balta is extremely faulty, but there is a much less pardonable error here in representing the northern passage into the sound as a wide and clear opening, whereas it is very much narrowed both by the position of Balta, and by a boundary of low rocks.

Of Yell, it is sufficient to say that the general outline is no less incorrect throughout, than are the draughts of the harbours; and, on the whole of Yell Sound, the incorrectness of which was already noticed in speaking of the smaller islands, it is proper to remark, that the want of an indicated tract renders the chart in this place nearly useless.

Having already observed upon the incorrect outline of the east side of North Maven, I need only here add that a singular omission will be found near Fedaland Point, which is indeed quite unaccountable, if ever any survey of this shore was made. The extremity of North Maven here forms a peninsula of considerable dimensions, separated from the main land by a beach of shingle and rocks; the indentation on the east side forming a cove for boats, and containing a very important fishing station. The omission of two fresh-water lakes in this neighbourhood, will perhaps not be considered of any moment; but, where some of these are laid down, it is a natural expectation that the whole should equally be found.

On the west-side of North Maven, the land under Ronas Hill is carried too far to the westward; and the same sort of incorrectness prevades the whole of this shore. Independently of the omissions of the two bays formerly noticed at Hillswick, there is a third sandy inlet on the west side of that peninsula, where a boundary of rocky cliffs is indicated in the chart. In the same manner, there is not due value given, either to the dimensions or form of Hamers Voe, which, as I formerly observed, is a very important object to vessels embayed in St. Magnus's Bay. Respecting the outlines from this part as far as Scalloway, the chief remarks, as far as the use of the chart is concerned, have already been anticipated in speaking of the various anchorages; and it would be superfluous to enter into more details respecting so erroneous a specimen of geography as it exhibits. Yet it may be remarked, that the very incorrect outline of the two Burras is particularly unpardonable; as the survey of Collins, to which the compiler might have had access, gives a far more correct view of them. It is indeed difficult to conceive how any one drawing them at all, could have contrived to do it so incorrectly; as the most superficial examination by the eye, is sufficient to correct the glaring errors which occur in the chart. The bridge which connects the two is not only omitted, but its place is indicated much too far to the southward. A deep bay on the east side of East Burra, which is reduced to almost nothing in the chart, is so situated and of such dimensions in nature, as to tempt a boat into it from Cliff Sound, with the expectation of finding the passage between the two islands. At the southern extremity of each of the islands, there is also a peninsula separated by a low rock, in one case indeed nearly insulated; but these are totally omitted in the chart.

But it is unnecessary to proceed further, and I shall therefore conclude these remarks on the general outlines of the Shetland Chart, by noticing the incorrectness of the draught of Foula, which the most superficial view might have given in a better form, and which it would require no great expense of time to survey with sufficient accuracy for all general purposes.

In terminating this communication, I shall only add, that a set of the most common astronomical observations on the latitudes and longitudes of a few leading points, would materially improve this chart; and that, with the assistance of the pilot before-mentioned, the industry of an active person would, in one or two summers, supply most of the chief desiderata in the present imperfect documents, until a real survey of all the islands can be effected. It is in vain to attempt to construct such a chart by the ordinary operation of boat surveying; as the rapidity and uncertainty of the currents, render it absolutely impossible to determine points in this manner with any tolerable accuracy. I shall therefore trespass no longer on the patience of the readers of this Journal, to most of whom the navigation of Shetland is, perhaps, as little interesting as that of the Celebes or Loo Choo. It is sufficient to have justified, by the details already given, the general censure with which I commenced this communication; and its object will be fully attained if it shall either

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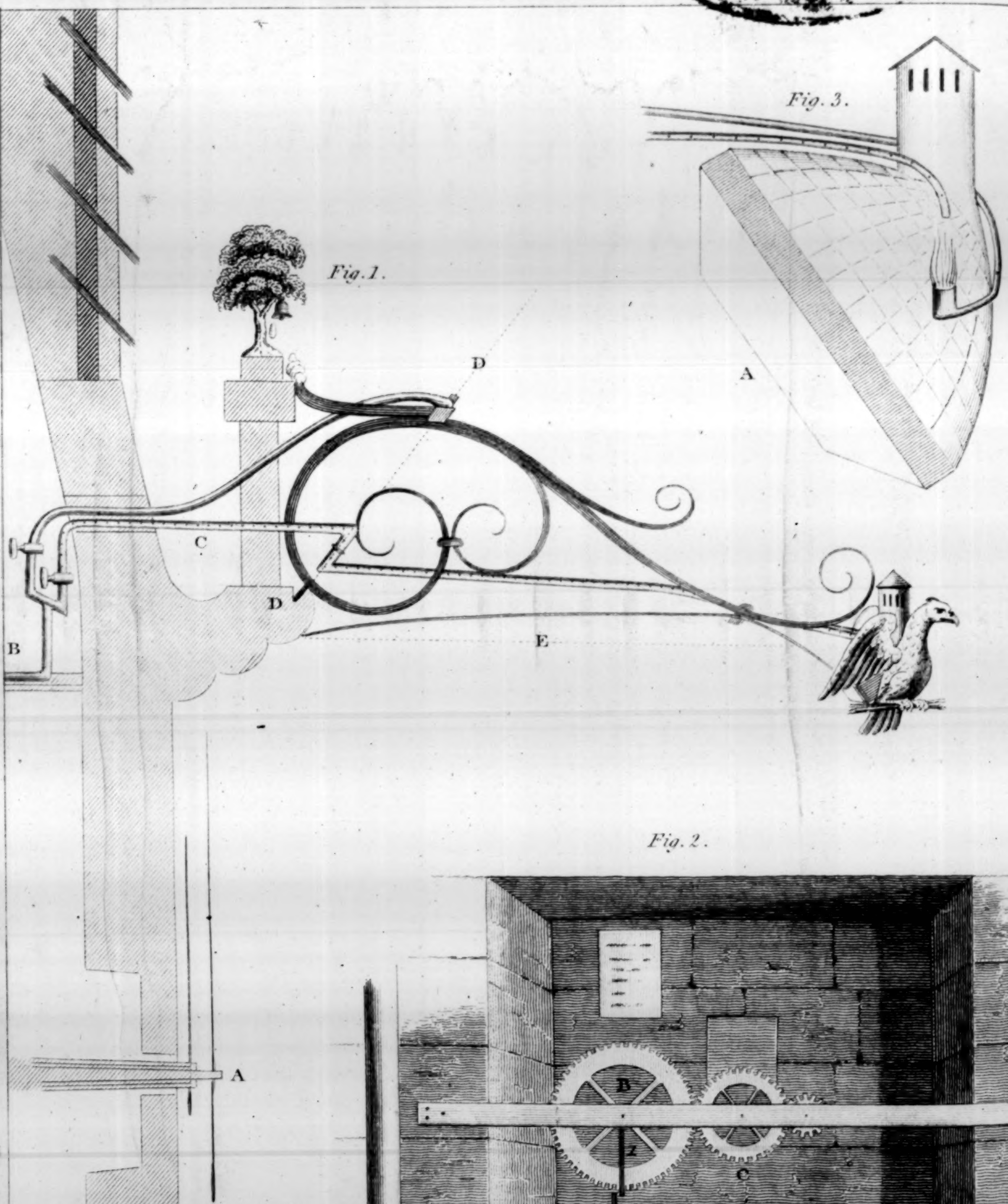

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induce those who may with authority undertake it, to commence this necessary work; or point out to unemployed officers in these times of peace, a subject, from which, if they do not reap profit or fame, they will assuredly acquire the thanks of many who, like myself, have hourly hazarded their lives during a stormy summer in a dangerous and anxious navigation.

Shetland, August, 1821.

ART. III. *Account of the Method of illuminating the Clock Dial on the Steeple of the Tron Church in Glasgow.*

[We are surprised that no attempt has yet been made to illuminate the dials of the London steeples, more especially as gas has been conducted into several of our churches, and is almost always abundantly laid on in the vicinity, for the purpose of lighting up church-yards, and thus preventing the unhallowed visitations of a class of people, who, under powerful patronage disgrace this metropolis and its suburbs, commonly called *resurrection men.* Several of our city clocks are, as it were, constructed for the purpose; such as those of St. Dunstan's in Fleet-street, of Bow church in Cheapside, and others which project after the like fashion. The Parish Church of St. James is also very advantageously situated for nocturnal illumination, one of its four sides being seen from a number of streets at the west end of the town.

The following is a description of an ingenious plan successfully adopted at Glasgow, under the superintendence of Messrs. John and Robert Hart. We understand that the hours on the clock-dial, which fronts a long line of streets, are legible at night to as great a distance as when the sun is directly shining upon the tower.]

THE Tron steeple of Glasgow is of the Gothic order, consisting of a square tower surmounted with a pyramidal top; the square tower terminates with a rail, or balcony, beneath which the clock-dial is placed.

The gas is introduced into the bottom of the steeple and conveyed up the wall by an iron pipe of one inch bore: this pipe passes within eighteen inches of the wheel-work of one of the dials, and a wheel, of double the diameter of the hour-wheel, is so placed as to be driven by it; this, of course makes one revolution in twenty-four hours. The hours are engraved on the rim of this wheel, and a moveable arm attached to it, which can be fixed at any hour or half-hour by means of a clamp screw upon the centre, and a steady pin and holes in the rim; this arm serves to disengage a catch that holds up the hand of the main stopcock, so that by this means the clock shuts off the gas in the morning. A painted board with the respective months and hours serves to direct the person who has the charge of it, to shift the arm as the season alters. The pipe afterwards ascends and branches off to the respective dials.

The lamp is formed of a copper bowl, eighteen inches diameter, and of a parabolic figure, of three inches and a half focus: it is lined in the inside with small pieces of looking-glass, imbedded in white paint and putty, similar to some of the reflectors used for light-houses; the front is enclosed with glass. The burner used is a No. 3, Argand, with each alternate hole shut up, and equal to a No. 1, *Glasgow* Argand.

The lamp, properly inclined so as to illuminate the dial, is suspended about seven feet from the building, above the centre of the dial, the bottom of the lamp is on a line with the top of the dial plate. From the situation of the dial it was necessary to fix the lamp from the balcony; and that it might be easily got at, the branch is jointed so that it can be drawn up to the person within the rail, to be cleaned when necessary. It is lighted from the inside of the steeple by a *flash pipe*, or separate tube, pierced along one of its sides with holes, and partially covered to protect it from the weather; so that when the gas is admitted and a light applied at one end, each hole lights the next till the flame reaches the farthest extremity, and thus kindles the gas issuing from the burner; the *flash pipe* is then shut. The exterior of the lamp represents the Eagle of Jupiter armed with lightning, and the whole is surmounted with the city arms. From the cheerful appearance of this dial, it is likely that other clocks may likewise be lighted, where the building will admit: a simpler and neater method than that which the peculiarities of the Tron church steeple rendered necessary, might be adopted; namely, to cut a small circular window about two feet in diameter above the dial, from which the lamp might be put up and cleaned when necessary, and when lighted pushed forward: here a straight rod only would be required to suspend it, which might be run out over a wheel having a balance weight in the inside to return it again to the window in the morning. The back of the lamp might be made to represent the city arms, or any other appropriate device. When the clock disengaged the gas stopcock, it might also liberate the catch that retains the lamp, and allow it to run in, so that the person who lighted it, would only have to hook up the stopcock, light the lamp, and push it forward into its proper situation for illuminating the dial: a simple folding-piece of three joints, like a foot-rule, would connect the lamp or lamps to the centre or main gas pipe.

Description of the Plate.

Fig. 1. is a section of part of the steeple—A, the dial and hands—B, the gas pipe, with C, the flash pipe branching off—DD, Joint on which the branch turns when drawn up—E, stays for its support.

Fig. 2. shews the means of shutting off the gas—A, the ascending pipe and main stopcock open, with its hand upon the detent—B, the discharging wheel; 1, the moveable arm, 2, the detent—C, the hour-hand work of the dial.

Fig. 3. Section of the reflector and burner—A, glazed cover.

ART. IV. *On the Difference of the Functions in certain Nerves of the Face, illustrated by their Anatomy in the Inferior Animals, and by a comparison of their Uses in Man and Brutes. By* JOHN SHAW, Esq.

IN a paper published in the last Volume of the *Transactions of the Royal Society*, Mr. Charles Bell has shewn that two sets of nerves, differing in structure and function, go to the face in man. One of these has the property of controlling the actions of the muscles of the mouth and nostrils in breathing, and of giving expression to the features in emotion; while the other appears to be for the purpose of regulating the muscles in mastication, and those actions which are independent of respiration and expression, and for bestowing sensibility on the skin.

Having been engaged with Mr. Bell in making the experiments by which these facts were proved, my attention was naturally called to a further observation of the condition of the muscles of the face in the different kinds of paralysis. The cases which I have examined during this inquiry, have induced me to hope, that by following up the observations which led to the discovery of the difference between the functions of the two systems of nerves, we shall eventually be able to ascertain, the cause of the variety of symptoms in paralysis.

In one man, I could see no symptom of palsy until he laughed or sneezed; while in another, the features distorted by paralysis regained their proper balance when the actions of laughing or sneezing were excited. In a third, the paralysis was apparent, not only while the features were at rest, as in the case of common palsy, but the distortion of the countenance, instead of being diminished, was increased to a most unusual degree, when the patient laughed or sneezed.

Before attempting to account for the variety of the symptoms in these cases, I propose to give a description of the nerves of the face, and of the changes produced on the expression in different animals, by cutting a branch of either set of nerves.

Two nerves which arise from distinct parts of the brain, and which differ from each other in structure and function, are distributed on the face in man, and on the corresponding parts in the greater number of mammalia.

The one hitherto called *Portio Dura* of the VIIth. belongs to the system of *Additional*, or *Superadded Nerves*, and passes to every muscle that is in any way connected with respiration or expression; while the other, called the *Vth*, or *Trigeminus*, is one of the original, or symmetrical system, and goes not only to the same muscles, but also to the skin, and to the deeper muscles, whose action is principally that of mastication.

The proportion of the Facial Respiratory Nerve to the Vth. is greater in man, than in any other animal. If we descend to the next link in the chain of beings (the monkey,) we shall find the proportion of it to be much diminished, and that of the Vth increased. The distribution of the nerve is more complicated in the monkey than in the dog, its intricacy being apparently in proportion to the number of the muscles of expression. From the lion, the dog, and cat, we descend to the horse, ass, and cow; in these animals, there is a marked difference in the distribution of the nerve, from that of either the monkey or the dog, for, excepting a few branches, which pass to the muscles of the external ear, and to the eyelid, the whole of the respiratory nerve is confined to the muscles of the nostrils and side of the mouth, while in the carnivorous tribes it is spread in great profusion overthe cheeks and side of the neck.

There are, however, some varieties in the classes of graminivorous animals. In the gazelle, sheep, and deer, the distribution of the nerve is still more simple than in the horse; while in the camel it is more profuse, and is, in this respect, intermediate between that of the carnivorous and the graminivorous animals. The expression of the enraged camel is sufficiently ferocious, and the manner in which he shews his tusks, when dying, is very similar to that of a carnivorous creature*.

We are told by those who have seen an elephant in a rage, that he is most sublime and terrific; but the anatomy of the

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* The anatomy of the nervous system of the camel is very different from that of the greater number of quadrupeds.

We had an excellent opportunity of examining it last spring, in the courier camel, or maherry, which was brought from the interior of Africa, by Captain Lyon, as a present to his Majesty. In the dissection of this animal we noticed many interesting facts, which have been overlooked by comparative anatomists, and particularly the difference of the distribution of the nerves of the neck and stomach, from that of the same nerve in the horse and ass, and from that in the greater number of ruminating animals.

We found that the nerves of the neck, in their number and distribution, resembled those of a large bird, as the swan, &c., much more than those of a horse or bullock, and particularly in the spinal accessory, or superior respiratory of the trunk, being either deficient altogether, as in birds, or quite different to what it is in the greater number of quadrupeds. This anatomical fact is a strong proof of Mr. Bell being correct in the opinion which he has formed of the use of this nerve. While in Paris last September, I was told that there was a specimen of the brain of a camel, in

Portio Dura leads me to suspect that the expression of rage, however terrible, must be quite different from that of the ferocious snarl of the lion. In the face it must be in a great measure confined to the contortions of the proboscis, and to the eye, for, excepting a few branches to the eyelids, the distribution of the nerve of respiration and expression is confined almost entirely to the proboscis*.

M. Cuvier's collection, in which the spinal accessory nerve might be seen; but on examining the preparation, I could find no trace of the nerve. There were some little filaments pointed out to me, as the origin of the nerve, but to this I could not assent. If these fibres, (which, however, in the present state of the preparation are very obscure,) be compared with the origin of the spinal accessory, even in the sheep, we shall be forced to conclude that, if there be a nerve of this kind in the camel, it is so small that the truth of the opinion, as to the use of the nerve in other animals, cannot be affected.

* During the last winter I often visited the Menagerie in Exeter Change, to study the motions and uses of the proboscis of the elephant, and this I had a good opportunity of doing, as the small elephant there, was so gentle, that he permitted me to handle his trunk freely. From the great power which the elephant has over his trunk, as a machine, I was certain that there must be large nerves running to it, similar to those which supply the fingers in man; but as the proboscis forms an important part in the respiratory system of this animal, I thought that, in the dissection of it, there would be the most distinct proof of the accuracy or fallacy of Mr. Bell's opinions on the subject of the *Portio Dura*. The animal died in the month of May, and, through the kindness of my friend Mr. Mayo, I was enabled to make an examination of the nerves of the trunk. The dissection was most satisfactory, for the trunk was found to be supplied, not only by branches of the Vth. pair, as described by Cuvier, but also by a very large branch from the *Portio Dura*.

The *Portio Dura* in this elephant was found emerging from the parotid gland, as in other mammalia. It gave off some descending branches to the neck, but passed from behind the jaw to the proboscis, almost as an entire nerve, and of the size of the sciatic nerve in man: in its course it had only given some small branches to the muscles of the eye, to those of the ear, and to a small muscle which corresponds with the platysma. Before it passed into the substance of the proboscis, it united with the second division of the Vth. pair, which comes forward from the infra orbital hole, in two large branches. The two nerves being then closely united, passed between the layers of the muscles, which form the greater

If we compare the anatomy of the facial respiratory nerve, in the various classes of birds, we shall find its distribution to be analogous to that of the same nerve in the different tribes of quadrupeds. In the game-cock, a few branches of the nerve pass to the loose skin under the jaw, which is dilated in crowing, the greater number being distributed on the muscles of the neck, which causes the elevation of the feathers when he puts himself in an attitude for fighting. But in the duck, which, when enraged, has little or no power of expression, the same nerve is not larger than a cambric thread, and passes only to the skin under the jaw.

The effect upon the muscles of the face, produced by cutting the facial respiratory nerve, is in the ratio of the intricacy of the distribution of the branches, and according to the proximity of the part cut, to the origin of the nerve from the brain.

I cannot detail any examples of the effect of cutting this nerve in man, for, though I have witnessed operations where it was done, yet, as they occurred before I was acquainted with the facts now established, I did not take notes of the consequences which ensued. However, I shall presently detail cases where, by disease or accident, the whole nerve in some instances, and part of it in others, has been injured. In those cases the symptoms very nearly corresponded with the phenomena presented, when the whole, or only a part, of the nerve has been divided in animals.

About four months ago, I divided the left facial respiratory nerve of the most expressive monkey I could find in the

mass of the trunk. The *Portio Dura* became quickly diminished in size, as it gave off its branches in great profusion to the muscles: but the Vth. was continued down, as a very large nerve, to nearly the extremity of the trunk; in this respect resembling the nerves to the fingers in man. On making sections of the proboscis, near its extremity, a great number of these nerves were seen in its substance.

A few branches of the *Portio Dura* ran to the valvular apparatus in the upper part of the trunk; but this peculiar structure was supplied principally by a branch from the Vth. pair, which winded round under the orbit.

Menagerie in Exeter Change. The effect was immediate; his power of expression being destroyed on one side. When he was irritated, he snarled and showed his teeth on the right side only. During the first month, he could not shut the left eye, but of late, though he is able to close the eye, he has so little power over the motions of the eyelid, that when he is attacked with a stick, the orbicularis muscle appears to become so convulsed as to render the eye useless. He then seems to avoid winking with the other eye, that he may be on his guard.

The effect of injury to this nerve is more distinctly shewn on the human face. In the case of a little girl, which will be particularly related in another paper, the consequence of disease of the right *Portio Dura* is very striking. When she laughs heartily, the right cheek and the same side of the mouth are unmoved, while the muscles of the left side are convulsed with laughter.

If told to endeavour to laugh with the right side, she raises the angle of the mouth, but by an action which is evidently regulated by the branches of the Vth. nerve. This attempt to laugh gives a peculiarly droll expression to her face, and I think it is the same action which amuses us so much in the face of the famous mimick, who invites the public to see him *at home* every spring.

After having made the experiments on the *Portio Dura* of several animals, and observed the effect upon the human countenance, where the nerve had been injured, I was so much struck with the resemblance in the action of some of the muscles, to those of the actor alluded to, that I went to the Theatre to observe the expression of his face. Although there were evident marks of paralysis of the *Portio Dura*, there was a considerable degree of expression exhibited on the same side, for which I could not at the time account. It, however, appears to be now explained by the state of the little girl's cheek, for when she attempts to laugh with the right side, the expression is so similar, that it almost amounts to a proof that this performer has, by practice, gained such a power over the actions regulated by the Vth., as to be able to bring them into a state similar to that in natural laughter. But as this is done through the Vth., instead of the VIIth., it makes the expression on one side quite different from that on the other, and consequently gives a peculiarly ludicrous appearance to the whole countenance.

The experiment of cutting the facial respiratory nerve was performed on a dog. The following is the note made a few days after the nerve was cut: The dog is now quite well, having suffered very little from the operation; when he fawns, the right side of his face is completely motionless; (the nerve of the right side was cut.) When I threaten to strike him, although there is a tremulous motion expressive of fear in all the muscles of the left side of the face, the other is perfectly still; he cannot even close the eyelid, and instead of winking when he expects to be struck, the eyeball itself is turned up. When he is excited, there is an expression of alacrity in all the muscles of the left side of the face, and a brilliancy in the left eye, while the right is perfectly inanimate. This is shewn in an extraordinary degree when he is fighting with another dog.

The difference between the two ears is not so distinctly marked, for though the left ear be more elevated than the right, still there is an expression of alacrity in the erection of both ears. However, at this I was not surprised, as I had found, in a dissection which I had made of a dog, previous to performing the experiment, that the principal branches, which pass to the muscles of the ear, were so deeply situated, that to have cut them, would have probably endangered the animal's life. The effect upon the respiratory muscles of the right nostril, was not so distinct as in the experiment upon the ass, (which will be detailed presently,) but the power of giving that peculiar twist to the nose, which is so distinctly seen in a pointer setting, was destroyed.

This dog, in the course of two months, perfectly recovered the use of all the muscles which had been paralyzed; but this circumstance involves the question of the re-union of nerves, upon which I shall not at present enter.

I repeated the experiment on another dog in the month of May, and at the same time cut the infra orbital nerve of the opposite side. This dog is still in very good health, and now affords as striking an example of the effect of cutting the respiratory nerve of one side, and a branch of the Vth. of the other, as he did on the day, after the operation was performed. The effects produced by cutting the Vth. will be detailed presently.

I cut the same nerve of a cat, as near its exit from the stylomastoid foramen as I could. When she was irritated, she afforded an excellent example of paralysis of the action of the muscles regulated by this respiratory nerve. She spate with that side only, on which the nerve was entire. Here I succeeded in paralyzing the muscles of the ear; for, while she spate, the ear of the side on which the nerve was entire, was pulled back, while the other stood erect and motionless.

The same experiment is easily made on the ass, or it may at any time be done on an ox in the slaughter-house, for while the animal is insensible, and dying from loss of blood, we have only to divide the nerve, as it passes from before the ear; the convulsed motion of the nostril of the same side will immediately cease.

The effect upon the nostril is the most obvious symptom, when the nerve is cut in the ass*. If, after having cut the right

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* I performed this experiment on a horse at Charenton, at the request of M. Majendie, and with the assistance of M. Dupuy, Professor of the Veterinary College there. As I was anxious to execute the experiment quickly, and avoid the hemorrhage which is generally a consequence of seeking for the nerve in the middle of the parotid gland, I cut through the skin anterior to the jugum, in the hopes of immediately finding the nerve. A large branch, which I supposed to be the *Portio Dura*, was exposed, but on cutting it through, no paralysis of the nostril ensued.

There was naturally a degree of incredulity on the part of those who were spectators, as to the fact of the actions of the nostril being paralyzed by cutting the *Portio Dura.* But on saying to the gentlemen present, among whom was Dr. Spurzheim, that this being the first time I had made the experiment on a horse, I was afraid I had not cut the principal branch of the *Portio Dura,* they, with great liberality, afforded me every assistance. On examining the wound more particularly, I discovered another large nerve, and on cutting this the nostril was immediately paralyzed.

As I had some difficulty in understanding why the result of this experinerve, we hold the nostrils for a short time, so as to prevent the animal from breathing, he will, when freed, begin to snort, but with the left nostril only. If we hold carbonate of ammonia to the paralyzed nostril, he will not be affected; but if it be

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ment was at first so different from those which we had made in London on the ass, and in which the nerves are so similar to those of the horse, I took the first opportunity of again examining the anatomy of the nerve. I then discovered a good reason for my failure, as a large branch which passes from the second division of the Vth. pair after running for a short way, parallel to to the *Portio Dura*, joins with it. It was this branch I had cut in the first part of the experiment.

I have since, with the assistance of my friend Mr. Cæsar Hawkins, repeated the experiment on the horse, and which, I am happy to say, can be done without giving the animal any pain, for the actions of the nostrils, and the irritability of the nerves, especially of the respiratory class, continue so long after the animal is insensible, that, by cutting the *Portio Dura* we may stop the convulsions of the nostrils, and afterwards, by exciting the nerve with the galvanic forceps, exhibit the peculiar set of actions regulated by it.

The experiment was made in the following manner:

As soon as the animal became insensible from loss of blood, I cut through the skin over the jugum, so as to expose the nerves passing down to the cheek and nostrils The nostrils, during this time, were in convulsive action, and corresponded with that of the chest. On cutting across the superior branch (which though principally of the Vth., had some branches of the VIIth. intermingled with it at the part cut,) scarcely any change was perceived; but on cutting the lower branch, which is almost entirely formed by the *Portio Dura*, the convulsive actions of the muscles moving the nostril immediately ceased. When this was repeated on the other side of the face, the same consequences followed. On afterwards irritating the extremities of the nerves with the galvanic forceps, the actions of the muscles of the nostrils were excited when the inferior branch was touched, but little or no effect was produced on touching the upper (the branch of the Vth.)

These circumstances afford a strong proof of the necessity of paying particular attention to the anatomy of the nerves, before we draw any conclusions from experiments upon them.

I may, however, observe that, although there was in this instance a degree of culpability, all the experiments which have been made in the present investigation have been founded on views taken from comparative anatomy, and from observations made on the habits of the various classes of animals; by following this plan the experiments have been seldom or never attended with conflicting results.

held to the other, he will snuff it up, and then curl the nostril, and have an expression in the whole of this side of the face, as if he were going to sneeze, while the right will remain quite unmoved.

After discovering that the plexus of the *Portio Dura* passed to the muscle which moves the feathers of the neck in some birds, I cut the nerve of one side in a game-cock. On opposing him to another cock, there was a marked difference in the erection of the feathers of the two sides; but as the action of the muscles of the entire side appeared to operate on the feathers of the other, I attempted to cut the corresponding nerve, but while doing this, the hook, by a sudden struggle of the bird, became entangled with the branches of the par vagum, and other nerves of the neck, which lie close upon the *Portio Dura*, and the animal immediately expired. I have not since repeated the experiment.

Though I have generally used the old nomenclature, *Portio Dura of the Vth*, yet I believe it to be quite an error to consider this nerve to be it in any way connectedwith the auditory nerve. In the duck, the distinction between the two is most complete, there not being the slightest connexion between them. Comparative anatomy would induce us rather to consider the *Portio Dura* as connected with the VIIIth., than with the *Portio Mollis.* Perhaps the name of "Respiratory Nerve, to the muscles of the face," as given by Mr. Bell, is the most appropriate, as it is indicative of its principal functions.

Comparative Anatomy of the Vth *Pair.*

This nerve is, in every respect, different from the facial respiratory nerve. The observations which have been made on its minute anatomy in man, and in the lower classes of animals, and the result of experiments, warrant the opinion formed by Mr. Bell; *viz.*, that the Vth pair is similar to those nerves which arise from the spinal marrow, and which, in their origin and distribntion, are so essentially different from the class of respiratory nerves.

The rudiments of the Vth may be discovered in the lowest classes of animals. If a feeler of any kind project from the head of an animal, be it the antenna of a lobster, the *moustache* of a phoca, or the trunk of an elephant, it is a branch of the Vth which supplies sensibility to the member, and regulates the voluntary actions of its muscles. Thus it may be compared to those nerves in man, which pass to the muscles of the arm, and to the tips of the fingers.

But this nerve is also, in the greater number of animals, connected with the organ of taste—and consequently it is very large; its magnitude being in proportion to the size of the apparatus connected with mastication and taste. Thus, in the lower scale of animals, the nerve is much larger in proportion than in man; indeed, its size, compared to that of the *Portio Dura,* may give us a better estimate of the comparative degree of the power of expression, than can be deduced from any other fact of anatomy. A good example is afforded in the goose or duck. In the latter bird, the six branches of the Vth, when laid together, form a mass equal in size to that of the largest nerve in a man's arm; while all the branches of the *Portio Dura* would not form a nerve larger than a common sewing thread.

In the cat, and in the hare, the branches of the Vth pass not only to the muscles, but also into the whiskers; while the branches of the facial respiratory nerve go past the hairs, and enter into the muscles, moving the tip of the nostril. It is rather difficult to demonstrate the nerves going into the bulb of the hair in these smaller animals, but it is easily done in the phoca. A preparation illustrative of this fact was shewn to me some years ago in Amsterdam, by Professor Vrolich; and in the first number of the *Journal de Physiologie Experimentale,* by M. Magendie, there is an account of "les Nerfs qui se portent aux moustaches du Phoque," by M. Audral. This fact of anatomy, which has been denied by some, is farther demonstrated by the dissection of those animals which have tufts of hair, or whiskers, over the eye. In the American squirrel I have traced branches of the first division of the Vth into the bulbs of the hairs over its eye-brow.

It will be unnecessary to give more proofs from anatomy of the distribution and *use* of this nerve being different from that of the *Portio Dura.* I shall now corroborate them by mentioning the result of two experiments.

In the same ass, of which the *Portio Dura,* or facial respiratory nerve of the right side, had been cut, the infra orbital branch of the Vth of the left side was divided.

Upon cutting this nerve no change was produced in the action of the muscles moving the left nostril in respiration, and this could be easily observed, as the other nostril had been paralysed, by cutting the *Portio Dura* of the right side. The effect, however, upon the muscles of the lip during feeding was very distinct, for although the same muscles were not paralysed in the act of respiration, still they were rendered incapable of mastication and voluntary motion, as the animal could no longer use this side of his lip in gathering his food *. The sensibility of this side was destroyed, for the animal did not start when it was pricked with a needle, as he did when the other was touched. I performed a similar experiment on a dog, which is still alive. When he is quiet, the nose is twisted to the side upon which the Vth is entire, and the *Portio Dura* deficient, but the moment he is excited, the nose is pulled to the other side.

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* In an account which M. Magendie has given of some experiments similar to those detailed in this paper, which he repeated at Charenton, he says, " Le résultat que nous avons obtenu s'accorde parfaitement avec celui que nous venons de rapporter, à l'exception toutefois, de l'influence de la section du sous orbitaire sur la mastication, influence qui n'a pas été évidente pour moi."

The idea that we had, in our experiments in England, found the power of mastication destroyed, must have arisen from the difficulty I found of expressing myself correctly in French, when discussing the results of the experiments with M. Magendie.—There is *one act* of mastication destroyed by cutting the infra orbital nerve, but to destroy the power altogether, it would be necessary to cut through the *trunk* of the Vth nerve of each side.

The difference of the degree of sensibility in the two sides corresponds with that found in the experiment on the ass.

A description of several different kinds of palsy will be given in my next communication, to prove that the changes produced on the human countenance in palsies, depend on the particular system of nerves diseased, and that the two systems are seldom or never affected at the same time. It will also be shewn, that, by exciting the actions regulated by the one system of nerves, the distortion caused by paralysis of the other will disappear.

By a knowledge of these facts, it may be expected we shall not only be able to form a more correct diagnosis of the nature and seat of palsy than heretofore, but also to estimate the degree of danger attending each class of symptoms.

Albany, London, Dec. 1, 1821.

ART. V. *Letter from* DR. COINDET, *of Geneva, to the Editor of the Journal of Science, on the administration of Iodine in Scrofula**.

WHEN I published my second Memoir, I already possessed a great number of good observations on what I regard to be the complement of my discovery, that is, the employment of iodine by friction in strumous swellings, and other scrofulous affections. An experience, much extended and varied since, has confirmed me fully in the belief that iodine is the specific of this class of maladies, against which we possessed only secondary remedies, namely, those which act indirectly, whilst I find that this excites directly and exclusively the lymphatic system, whence we obtain surprising success in the above cases.

I prescribe a *pommade*, consisting of hog's-lard an ounce and a-half, white wax two drachms, fused together, and united by trituration with 36 grains of hydriodate of potash, or of soda. The size of a hazel-nut of this ointment to be rubbed in every morning and evening, on the goitre, gland, swelled joint, chronic tumour, &c. Various effects result from this practice. One of these is a local irritation, or chapping of the skin, an inconvenience to be avoided. If there be a chronic phlegmasia in the scrofula of the neck, or in any of these strumous swellings, characterized by hardness, lancinating pains, we must stop the above treatment till, by leeches, emollient cataplasms, ipecacuan, and saline purgatives, we have paved the way for the success of the iodic frictions. But it happens pretty frequently that, in their turn, these frictions produce a sort of phlegmasia in the same swellings, that is, after a certain number of applications the ointment developes an excitement of the lymphatics, characterized by hardness, pain, and extreme sensibility of the scrofulous glands. We then must immediately suspend all friction, and pursue the antiphlogistic plan, sometimes very actively. Whether it be that the nature of the inflammation has been modified, or that the absorption alone has been powerfully augmented, the fact is, that the pains give way, and, after a few days, we are surprised to find that the obstructed glands are softened, smaller, and that the cure has been accelerated by that accidental disorder.

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* Though most of the facts communicated in this letter have already appeared in our Journal, we have no doubt that our medico-chemical readers will peruse it with interest.

It happens also, but much more rarely, that when the iodine has been prescribed inwardly, there arises a constitutional action (agreeably to what I have stated in one of my Memoirs,) which we must regard, as an iodic super-saturation of the system. The patient becomes pale, he takes a complexion peculiar to the effects of the iodine, he turns lean, first of all where the frictions are applied, especially if that be the neck, and then the emaciation extends over the whole body, if the frictions be continued. The pulse becomes occasionally frequent, but at the same time the goitres, or scrofulous glands, dissolve during this action with great rapidity, so that in some cases the cure excites astonishment. Yet, by these means, we make the patient suffer unnecessary inconvenience and risk, injuring the reputation of this powerful medicine to no purpose. As soon as we perceive the glands, tumours, &c. soften and subside, we must suspend the frictions, and resume them after the interval of a few days.

I have ascertained, in a precise and certain manner, that the action of iodine continues some time after its administration is stopped. It is therefore absurd merely to say, as I have heard some " You have the goitre, take iodine." These scrofulous obstructions demand a careful treatment, which must be observed for some time, both internally and externally, with regard to this remedy. It, in fact, requires attention somewhat similar to those which we give to corrosive sublimate, when prescribed in syphilitic cases.

I continue always to prescribe iodine internally, but less frequently than I formerly did, because the frictions give the same results, without running the hazard of injuring the stomach ; and it is a singular fact, that half a drachm, or 36 grains, of hydriodate of potash, under the form of inunction, cures or dissipates a goitre, or scrofulous tumours in the neck, in the very same space of time that the following solution does : Hydriodate of potash or soda 36 grains, iodine 10 grains, distilled water 1 ounce, which are united together. Of this solution 10 drops are taken three times a-day at first, and then gradually augmented in quantity.

I shall be particularly obliged to you, Sir, for communicating this letter to my medical brethren in Great Britain ; and I remain your most obedient servant,

Z. Coindet, M.D.

Geneva, September, 1821.

ART. VI. *Process for procuring pure Platinum, Palladium, Rhodium, Iridium, and Osmium, from the Ores of Platinum. By* M. BARUEL, *Chemical Operator in the School of Medicine at Paris.*

Communicated by the Author*.—With Supplementary Remarks by the Translator.

1. Two sorts of platinum ore occur in commerce, one of which is white and brilliant, the other is blackish coloured. The latter contains much more iron † than the preceding; both ores exist always in the form of small spangles, which vary in size; platinum ore is one of the most compound known; besides the five metals above noted, several others are found in it, especially two kinds of ferruginous sand, one of them attractible by the magnet, the other not, and which is a combination of the oxides of titanium and iron; there is besides chromate of iron, some copper, particles of gold alloyed with silver, with copper, and mercury. It contains, moreover, some sulphuret of lead and copper. We may hence judge of the singular complexity of this mineral, and be ready to acknowledge that its exact analysis, in regard to the proportion of its constituents, is nearly impossible. In order to separate the platinum, palladium, rhodium, iridium, and osmium, from each other, and the rest of the bodies, the following method is the one which long experience has proved most successful.

2. The ore is triturated in a cast-iron mortar for a considerable time, during which a stream of water is constantly passed over it, to wash away the ferriferous sand, the titanite, and chromate of iron, reduced to an impalpable powder. When the ore is very brilliant it is left to settle for an instant; the

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* This valuable memoir derives peculiar interest from the large importation of the above ore, daily expected from South America, in consequence of the negociation between M. Zea and some London merchants.

† Rather the fine black powder, or ore of iridium and osmium, noticed in paragraph 8.—TR.

water is decanted off, and it is then exposed in a crucible to a red heat during a quarter of an hour. The whole mercury is thus volatilized, when we can readily distinguish the spangles of alloy of gold and copper by their colours.

3. The calcined ore being introduced into a tubulated retort, we pour over it half its weight of nitro-muriatic acid (aqua regia) composed of one part of nitric acid, at 25° Baumé (1.210 sp. gr.) and three parts of muriatic acid, at 18° (1.14), and heat the mixture for half an hour. Such acid dissolves all the gold, all the lead, the greater part of the copper, and a very small quantity of platinum, palladium, and iron, while the silver is converted into a chloride, which remains mingled with the ore not attacked. After decanting the acid liquor, the ore is thrown on a filter, and washed with a sufficient quantity of water. The filter-funnel being transferred to another vessel, the filter is to be washed with a very weak water of ammonia. By this means we dissolve all the chloride of silver, which is recovered by saturating the filtered liquor with muriatic acid.

4. The solution which contains the gold, lead, copper, and iron, with a small quantity of palladium and platinum, being added to the water which has served for the washings, the whole is now evaporated to the consistence of syrup, which is diluted with thrice its volume of water, and treated with sulphuric acid, drop by drop, to precipitate the lead in the state of sulphate, to be afterwards separated by the filter.

5. Into the filtered liquor a solution of proto-sulphate of iron must be poured, which throws down the gold and palladium in the metallic state. We decant the liquor, wash and dry the precipitated metals. The platinum remains in the liquor with the iron and copper. We concentrate this liquor by evaporation, then pour into it a sufficient quantity of a saturated solution of muriate of ammonia, which throws down the platinum in the state of ammonio-muriate. This must be washed on a filter and dried.

6. The gold may be very easily separated from the palladium by melting these metals with four times their weight of silver, and acting on the alloy with concentrated nitric acid, which

dissolves the palladium and silver, but leaves the gold in the form of a brown powder, which may fused into a button in a crucible. Into the nitric solution of silver and palladium we pour muriatic acid, which throws down all the silver in the state of chloride. The liquid freed by the filter from the chloride contains only palladium. We add to it a few drops of solution of sal-ammoniac, then saturate the redundant acid by ammonia; the whole palladium is thus precipitated in the state of an ammonia proto-submuriate of palladium, which exhibits small needles, of a delicate rose colour. This salt is to be washed on the filter, and dried.

7. The ore of platinum which has been successively treated with weak nitro-muriatic acid, and then with ammoniacal water, to carry off the chloride of silver, is to be strongly desiccated. Having replaced it in the retort, we pour over it a weight equal to its own of nitro-muriatic acid, made in the same proportion as the above, but with this difference, that the acids ought to be as concentrated as possible. I employ for this purpose nitric acid, at 40° (1.387 sp. gr.) and muriatic acid, at 23½° (1.195). The retort is placed on a sand-bath, with a tubulated receiver adapted to its neck, and it is heated moderately. A brisk effervescence soon arises, owing to the disengagement of much nitrous vapour, and a little chlorine. The action of the heat must be so modified as to produce the most beneficial effect on the solution, without volatilizing the acid. Finally, when the effervescence ceases, the fire is to be augmented till the liquid boils, and till no more orange nitrous fumes are disengaged.

When the action of the acid is quite exhausted, we decant the liquid into a matrass, and pour on the portion of the ore not attacked the same nitro-muriatic acid, equal in quantity to the first.

The mixture is to be heated anew, observing the same precautions as for the preceding solution. Finally, we treat the ore five times in succession with the compound acid. By this process six parts of this acid are sufficient to dissolve the whole platinum, palladium, and rhodium contained in the ore.

8. After the last digestion, which yields only a slightly

reddish-coloured solution, there remains a residuum, under the form of a brilliant blackish powder, which consists of an alloy of iridium and osmium. One part of this is a fine powder (see Note to paragraph 1,) and the other forms brilliant spangles. We shall return, in the sequel, to the residuum; let us employ ourselves at present on the solution.

9. We have said that all the platinum, rhodium, and palladium, were dissolved; but the acid also dissolves a little iridium and osmium, as well as the iron alloyed with the platinum grains. During the action of the acid on the ore, at the same time that the nitrous gas and chlorine are evolved, there is volatilized a little water and muriatic acid, which carry over with them a notable quantity of oxide of osmium, which is condensed in the receiver.

10. All the successive solutions of the ore of platinum are united and introduced into a retort of proper capacity, to which the receiver containing the former condensed vapours is attached. The retort is now heated on a sand-bath, till its contents acquire the consistence of syrup. By this means we drive off all the excess of the acid, which carries along with it into the receiver the whole oxide of osmium which that solution contained.

11. The product of the last distillation being saturated with lime, we distil over to one-half the volume. The product of this new distillation has an extremely penetrating odour, on account of the large proportion of oxide of osmium which it contains. It must be preserved in glass bottles, furnished with well-ground stoppers.

12. The concentrated solution of platinum is to be diluted with from five to six times its weight of water, then filtered.

13. The black powder which was not acted on by the nitromuriatic acid, is also to be washed with water, dried, and kept in a phial; we shall distinguish it by the name of the *black powder.*

14. Into the filtered solution we pour a saturated solution of muriate of ammonia, till this ceases to occasion any precipitate. In this operation there are formed ammonio-muriates of platinum, iridium, rhodium, and palladium. These two

last salts being very soluble, remain in the liquid with the iron, but the ammonio-muriates of platinum and iridium being very sparingly soluble form the precipitate, which has a tawny or reddish-yellow colour, of more or less depth, according as the proportion of the salt of iridium is more or less considerable. When the further addition of the muriate of ammonia produces no more precipitate, the whole is to be thrown on a filter of cotton, and washed with water of as great coldness as possible, which is conveniently procured by putting a bit of ice into the water intended for the washings. When the precipitate is sufficiently washed, which is recognised by the water that passes having merely a faint yellowish hue, it is to be dried. This precipitate, as we have remarked, is an ammonia-muriate of platinum, the pure yellow of which is altered by its mixture with the ammonio-muriate of iridium, which is red.

15. This impure ammoniacal salt of platinum is calcined in a crucible, observing to heat the crucible at first in its upper part, in order to avoid the volatilization of a portion of the salt, without its being decomposed. The heat is to be pushed to redness, at which temperature it must be kept up for an hour. By this means the salts are decomposed, and there remains in the crucible only the platinum and iridium. To separate these two metals we put them into a retort, and dissolve them anew in the nitro-muriatic acid ; but in this case the nitric acid must be only at 28° (1.24), and the muriatic acid at 19° (1.15). Two and a-half parts of this acid suffice to dissolve one of platinum thus reduced, without affecting the iridium. This metal remains at the bottom of the liquor (which is of a fine orange-yellow colour,) under the form of a grey powder. On filtering, pure iridium remains above, which is to be washed and dried.

16. The solution of platinum must be precipitated once more by muriate of ammonia; and the fine yellow ammonio-muriate of platinum thus obtained, is to be reduced by strong calcination in a crucible, observing the precautions already indicated. The pure platinum remains in the crucible, under the form of a greyish-coloured spongy mass, which acquires metallic lustre by friction against any hard body.

17. As platinum can be fused only in small masses at a time, and at a flame supplied with oxygen gas, or the compound flame of oxygen and hydrogen, it cannot be melted on the large scale like most others. However, chemists have succeeded in forming this metal into ingots of a very considerable weight, by uniting the particles with strong pressure at a very high temperature. For this purpose, a certain quantity of platinum, resulting from the calcination of the triple ammoniacal salt, is compressed in a crucible; then more is successively introduced, even to the amount of 20 or 30 pounds. The crucible is then covered, and heated to whiteness. The platinum is now transferred as speedily as possible into a square steel matrix, (a strong hoop of steel, jointed, would answer equally well) and capable of opening into two pieces by means of hinges. On the top of the ignited mass, a steel mandril, adapted to the cavity of the matrix, is to be applied, which is to be rapidly driven home, by three or four blows of a strong coining screw-press. By this powerful pressure, which the spongy platinum experiences at a white heat, it diminishes greatly in bulk, and its particles already acquire a pretty strong cohesion. The matrix, or collar, is opened, the mass of platinum is removed to be heated anew in a crucible to a red-white heat, at a fire acted on by two good bellows. It is again introduced with the utmost celerity into the matrix, where it receives five or six blows of the fly-press. In the second operation, all the particles of the platinum are sufficiently approximated to form a homogenous mass, which may be thenceforth heated, without inconvenience, among naked charcoal, giving it the greatest possible heat, and condensing, with two blows of the press, each face of the ingot. In thus transferring the mass of platinum successively, from the forge to the press about thirty times, we obtain an ingot perfectly sound, possessed of great malleability and ductility. Platinum thus made into ingots, is delivered to the workmen, who fashion it like gold and silver; that is to say, all the pieces are stretched at first under the rolling-press, and then fashioned by the hammer, taking care to anneal it from time to time. Thus are prepared, in France, the great masses of platinum, with

which are fabricated the large alembics, destined for the concentration of sulphuric acid.

18. The mother-water, from which have been precipitated the ammonio-muriates of platinum and iridium by pouring muriate of ammonia into the solution of crude platinum, has a reddish-brown colour, and contains all the ammonio-muriates of palladium and rhodium, as well as a certain quantity of the ammonio-muriates of platinum and iridium; because, as we have observed, these salts are not completely insoluble. It contains, moreover, all the iron which was alloyed with the platinum, and sometimes a little copper, which has escaped the action of the first portion of nitro-muriatic acid which was poured on the ore to dissolve the gold. This mother-liquor is put into matrasses, and plates of iron are plunged into it. The iron precipitates all the metals (except the oxide of iron) under the form of a black powder. When the whole metallic matter is thrown down, which is known by the liquor assuming a green colour, the plates of iron are removed, after detaching from their surfaces the adhering powder. The liquor is decanted off, and thrown away. The black precipitate must be washed several times, till the water employed passes off tasteless. The powder is then treated with weak nitric acid, which dissolves the greatest part of the iron, which, by the effect of the precipitation, had been alloyed with these metals *, and which takes up also whatever copper may remain. The residuum is washed anew, and treated with nitro-muriatic acid, which dissolves all the platinum, palladium, rhodium, and remains of the iron; but does not affect the iridium, which remains pure at the bottom of the solution in the form of a black powder, or metallic spangles. The iridium, being separated by the filter, is then washed, dried, and united to that formerly obtained (15).

19. The liquors are now to be united, and evaporated to the consistence of syrup, to drive off the greater part of the

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* Or, *during the precipitation had fallen down in alloy with these metals.* The original words are, " fer, qui par l'effet de la précipitation, s'étoit allié avec ces métaux."

acid excess; then this is to be diluted with four or five times its weight of water, as cold as possible. Into this a solution of muriate of ammonia is to be poured, till it ceases to occasion a precipitate. What falls is an ammonio-muriate of platinum, which must be separated by filtration. The solution is then concentrated, and allowed to cool several times in succession, to separate all the ammoniacal salt of platinum which it may contain. When the liquid is completely deprived of platinum, or when it yields no longer the yellow precipitate, we dilute it with five or six parts of cold water; and it ought to have a sensible excess of acid. This, if wanting, may be supplied by adding a little of the muriatic. We then pour into it water of ammonia, drop by drop, but not so much as entirely to saturate the acid-excess. Immediately there is formed, in the liquid, a precipitate in the shape of small needles, delicate and shining, possessing a beautiful pale rose-colour. This crystalline precipitate is an ammonio-subprotomuriate of palladium. Since this salt is insoluble, there can remain none of it in the liquid. It may be separated by the filter, and washed with very cold water. By heating this salt to redness in a crucible, the palladium remains pure. It may be afterwards melted in a cavity of ignited charcoal, on which a stream of oxygen gas is made to play.

20. The liquid freed from the salt of palladium, possesses a fine currant-red colour, derived from the ammonio-muriate of rhodium, which it holds in solution, and which is very soluble. It contains, moreover, a little muriate of iron, and occasionally a little muriate of copper, when this metal has not been entirely dissolved by the first portion of nitro-muriatic acid, which was made to act on the ore, as has been stated above. There are two modes of treating this salt, to obtain pure rhodium. The first consists in evaporating this liquid, at a gentle heat, to dryness; and boiling the residuum several times along with absolute alcohol. The spirit dissolves all the muriate of iron and copper, with the excess of sal ammoniac, and does not affect the ammonio-muriate of rhodium, which remains in the form of a saline powder of a fine carmine-red colour By calcining this

salt to redness in a crucible, we decompose it, and the rhodium remains pure and perfectly metallic. The second means of obtaining the rhodium from the above liquid, consists in plunging into it plates of iron. The rhodium and the copper are precipitated, carrying down with them a little iron. When every thing is fallen down, the liquor is decanted, the precipitate is washed, and boiled with an excess of strong muriatic acid, which dissolves all the iron. The liquid is now poured off, the residuum is washed with a sufficient quantity of water, and is next boiled several times with concentrated nitric acid, which dissolves all the copper. The rhodium being completely insoluble in each of these acids separately, remains under the form of shining pellicles, which must be washed and dried. Rhodium being the most infusible of metals, cannot be melted but in small pieces, by the aid of a flame fed with oxygen gas, or by the compound flame of hydrogen and oxygen. (See Annotations *infra.*)

21. Let us return to the black powder separated from the platinum ore, by treating it with nitro-muriatic acid. We have said that this black powder was an alloy of osmium and iridium. It is scarcely affected by any nitro-muriatic acid. It requires, indeed, an enormous quantity of this acid to dissolve a minute particle of it. The only means of attacking this alloy, is to calcine it with nitrate of potash. With this view, we triturate the black powder with twice its weight of a mixture of three parts of nitre and one of caustic potash, and introduce the whole into a silver crucible, which is to be kept at a cherry-red heat for half an hour. In consequence of the affinity of the potash for the oxides of osmium and iridium, the nitric acid of the nitre is decomposed, and oxidizes these metals. The crucible is to be withdrawn from the fire, allowed to cool, and cold water is then poured on the materials. This dissolves the potash, the whole oxide of osmium, and a little of the oxide of iridium. The whole being thrown on a filter, the oxide of iridium remains above, which is to be washed and dried.

22. The filtered liquor which contains the combination of potash and oxide of osmium, as well as a little oxide of iridium, is

put into a flask, and saturated with nitric acid. The liquid is then put into a retort, to which is fitted a tubulated globe, surrounded with moistened cloths. On distilling, the water which rises in vapour carries with it all the oxide of osmium. When the liquid is two-thirds drawn over, the whole osmium is usually volatilized. The liquid remaining in the retort contains the nitrate of potash, and a trace of iridium. The aqueous solution of osmium is as colourless and limpid as distilled water. It has a strong and peculiar odour, extremely irritating to the nostrils, and which it is dangerous to inhale for any length of time. In order to obtain the osmium from this solution, it is put into a matrass, and we add a little muriatic acid to acidulate it slightly, and then insert a plate of pure zinc. The oxide of osmium is decomposed by the zinc, which is dissolved in the muriatic acid, and the osmium is precipitated to the bottom of the liquor in the form of a blackish-blue powder. When the oxide of osmium is completely decomposed, which may be recognised by the liquid losing its odour, we decant the fluid, pour the powder of osmium on a filter, wash it copiously with water, dry it, and put it immediately up in a well-stopped phial.

23. The oxide of iridium, proceeding from the calcination of the black powder with nitre and potash, which remained on the filter, is by no means pure. It is a mixture of oxide of iridium, of a certain quantity of the black powder, or alloy of osmium and iridium, which has not been affected by the nitre, and a little oxide of silver, derived from the crucible. This mixture is to be treated with nitro-muriatic acid, which dissolves only the oxide of iridium, converts the oxide of silver into a chloride, and does not act on the alloy. We next filter and wash. The unattacked alloy, and the chloride of silver, remain on the filter. This residuum is to be washed with water containing a little ammonia, which dissolves the chloride of silver, while the alloy of osmium and iridium remains pure. This may be again calcined with the mixture of nitre and potash, to decompose it completely.

24. Into the solution of iridium, which is of a very deep reddish-brown colour, muriate of ammonia is to be poured, and the

liquid is to be evaporated to dryness, at a gentle heat. The residuum is to be then treated with alcohol very highly rectified, which takes up the excess of sal ammoniac, and occasionally a little muriate of iron; because the alloy sometimes contains a little of this metal. When the alcohol is no longer coloured, the ammonio-muriate of iridium remains pure. It is necessary merely to calcine it strongly in a crucible to have pure iridium. This metal, being more infusible than rhodium, can be melted only in very small quantities by the oxygen on charcoal, or hydrogen blow-pipe.

Remarks by the Translator.

The preceding process was obligingly drawn up by M. Baruel, at the request of a chemical gentleman from this country, who wished to know the actual method now practised at Paris, for preparing the great masses of platinum of which Coturière forms his beautiful alembics. Permission was given to publish the process in a work on practical chemistry, now in preparation; but as the period of its appearance is a little uncertain, it has been thought fit to give it to the world in the present form. Though it be fundamentally the same with that published by M. Vauquelin in 1813, in the 88th volume of the *Annales de Chimie;* yet it is much superior in the clearness, method, and definiteness of the operations. These indeed are so explicit, that one of the most delicate processes of chemistry, is thus brought within the compass of almost every operative chemist. The paragraphs are here numbered, for reference in annotation.

The preliminary observations in § 2, are very useful; they are not given by M. Vauquelin. M. Baruel is judicious in prescribing, with M. Laugier, a retort and receiver for conducting the aqua-regia solutions. In all cases where nitric or nitro-muriatic acid is concerned, a great waste and annoyance are occasioned by the escape of acid vapours. These may be effectually saved, by connecting, with a somewhat long and wide tube, the first balloon to a second containing a very little water. The first balloon should have three apertures; one connected with the retort, a second in the same line connected with the receiver, and a third on the top, into which a small glass tube proceeding from a gasometer, filled with common air, is fixed. The tube which joins the two balloons, should have a slight declivity from the retort, and should dip at its end, into the water of the second. The outlet of this last must be closed with a Welter's tube of safety. When nitrous vapours are coming over open the stop-cock of the gasometer, and transmit into the first balloon, a moderate current of air. Its oxygen will immediately re-acidify the nitrous gas, which will condense in the form of nitric or nitrous acid, in the second receiver; while the azote will pass off. By M. Baruel's arrangement, § 7 and § 9, much of the osmium is preserved; which by following M. Vauquelin's directions is lost sight of, and dissipated.

In § 12, M. Baruel desires us to dilute with 5 or 6 parts of water, the concentrated platinum solution. M. Vauquelin, says that " ten parts of water and one of the solution in a state of great concentration, appear to me to be the best proportion." " Without the precaution of dilution," he observes, " it would be very difficult to wash the precipitate, and it would remain mixed with iron, and with the other metals, that happen to be present. It is better that the whole platinum should not be precipitated, than that the precipitate should be impure; because the platinum remaining in solution is separated in the subsequent processes." " Ammonio-muriate of platinum is not pure," he subjoins, " unless it has a lemon colour, does not become brown on drying, and is easily reduced to powder."

In a subsequent part of his interesting Memoir, M. Vauquelin states, that on treating the black metallic precipitate, (obtained by immersion of iron plates into the solution freed from platinum by sal-ammoniac,) successively with cold nitric, and muriatic acids, washing and drying the residuum, very acrid white vapours rose, which he ascertained, by heating a portion of the residuum in a crucible, to be a mixture of calomel and muriate of copper. " The sublimate," adds he, " contained likewise globules of mercury, and a black matter, which I suppose, from the smell which it exhaled, to be osmium." The fact is, that the chief part of the acrid vapours, was oxide of osmium, which was unnecessarily wasted.

The note at paragraph 18, has its commentary in the following observation of M. Vauquelin. "The muriatic acid employed after the nitric, contained likewise a great deal of iron, some copper and palladium, and even platinum and rhodium. This shews that a portion of these last three metals is precipitated by the iron in the state of oxide; otherwise muriatic acid would not have dissolved them. This seems to prove likewise that these metals in precipitating combine with iron and copper, and prevent them from being attacked by nitric acid, even when employed in considerable quantity." The great quantity of oxide of iron which precipitates along with the platinum, palladium, and rhodium, is very remarkable."

The process in § 15 and § 16, is a refinement on the former methods. M. Vauquelin contents himself with the first precipitate of platinum by sal-ammoniac, regarding it as quite pure. A great master of chemical analysis, to whom this process of M. Baruel was shewn, remarked, that his aqua-regia contained too much nitric acid; that if the suitable proportion of muriatic acid be employed, then the platinum may be thrown down pure, at first, by sal-ammoniac; and that M. Baruel dissolves by his menstruum, a portion of iridium.

The separation of palladium, under the form of an ammonia-submuriate in § 19, is precisely Vauquelin's method. Of this plan, M. Vauquelin speaks with complacency. "This process, more simple, and more exact than that of Wollaston, depends 1. On the insolubility of ammonio-muriate of palladium, even in water slightly acidulous. 2. On the solubility of the muriates of copper and iron in alcohol, and the insolubility of ammonia-muriate of rhodium in the same liquid." Yet the following extract from the same elaborate Memoir, seems to invalidate the above certainty of separation. "Now to obtain the rhodium, I concentrate the liquids from which the palladium has been separated, till they crystallize totally on cooling. I put the crystals aside to drain; they are *frequently* of two kinds and of two colours. Some have the form of hexahedral plates, and a fine ruby-red colour; while others, fewer in number, are square prisms of a yellowish-green colour. These last are ammonio-muriate of palladium." Thus we see that the ammonio-muriate of palladium has remained in solution, contrary to the statement above.

Dr. Wollaston's original method of separating rhodium from palladium and platinum is peculiarly elegant and economical, while it is perfectly exact. Into the solution deprived of the greater part of its platinum by sal-ammoniac, he immerses a piece of clean zinc. This throws down all the metals in the state of a black powder, except the iron, which remains in the solution. The copper and lead of the precipitate, weighing from 40 to 50 grains, being removed by very dilute nitric-acid, the remainder after being washed, was digested in dilute nitro-muriatic acid, which dissolved the greater part. "To this solution were added 20 grains of common salt, equal to one-fifth of the ore employed: and when the whole had been evaporated to dryness with a very gentle heat, the residuum, which I had found from prior experiments would consist of the soda-muriates of platina, palladium, and of rhodium, was washed repeatedly with small quantities of alcohol, till it came off nearly colourless. There remained a triple salt of rhodium, which by these means is freed from all impurities*." From the solution of this soda-muriate in water, zinc throws down the metal in a black powder, which may be agglutinated into a somewhat spongy metallic mass, by the heat of a very powerful furnace. Dr. Clarke, of Cambridge, is the only chemist, I believe, who has succeeded in giving to rhodium a high density, by fusion before his oxy-hydrogen blow-pipe. In an obliging communication to the present annotator, this gentleman states, that "Dr. Wollaston's own rhodium, taken from the pure specimen he gave me, is rendered perfectly malleable before the gas blow-pipe, and in this malleable state, after being hammered, its specific gravity, (estimated in distilled water, at the temperature of 60° of Farenheit,) equals 20.03, being as high as that of pure hammered platinum." Dr. Wollaston's rhodium, fused in his own air-furnace, has a specific gravity of only 9.74 by our experiments.

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* *Phil. Trans.* for 1804, page 423.

The separation of the salt of rhodium from those of platinum and palladium, as performed by Dr. Wollaston for the instruction of his scientific friends, is one of the most striking phenomena of chemical analysis. He mixes the three triple salts with a little water, in a watch glass, and evaporates to such a degree as leaves the mass apparently dry, yet with as much combined water as would give it the watery fusion at a higher heat. He pours on this a little alcohol, about specific gravity 0.850, and applies a gentle heat. Instantly after fusion, two very distinct and dissimilar fluid strata are formed; a red-coloured liquid above, and an oily-consistenced liquid below, of a dark-brown colour. The former is to be immediately poured off. The latter after being washed with a little more alcohol, yields a pure soda-muriate of rhodium.

The plan of separating palladium from its metallic associates in the platinum ore, originally invented by Dr. Wollaston, is very beautiful, and certainly in no respect inferior to that prescribed by M. Vauquelin. "To a solution of crude platina, whether rendered neutral by evaporation of redundant acid, or saturated by addition of potash, of soda, or ammonia, by lime or magnesia, by mercury, by copper or by iron, and also whether the platina has, or has not, been precipitated from the solution by sal-ammoniac, it is merely necessary to add a solution of prussiate of mercury, for the precipitation of the palladium. Generally for a few seconds, and sometimes for a few minutes, there will be no appearance of any precipitate; but in a short time, the whole solution becomes slightly turbid, and a flocculent precipitate is gradually formed, of a pale yellowish-white colour. This precipitate consists wholly of prussiate of palladium, and when heated will be found to yield that metal in a pure state, amounting to about four or five tenths per cent. upon the quantity of ore dissolved.

"The prussiate of mercury is peculiarly adapted to the precipitation of palladium, exclusive of all other metals, on account of the great affinity of mercury for the prussic acid, which in this case prevents the precipitation of iron or copper; but the proportion of mercury does not by any means influence the quantity of palladium, for I have in vain endeavoured, in the above experiment on crude platina, to obtain a larger quantity of palladium than I have stated, by using more of the prussiate of mercury, or to procure any precipitate by the same means from a solution of pure platina. The prussiate of mercury is consequently a test by which the presence of palladium may be detected in any of its solutions*."

In paragraph 22, M. Baruel directs to saturate the potash, in the alkaline compound of oxide of osmium, with nitric acid, and then to distil; but as the slightest excess of this acid would cause it to come over with the volatile oxide in the subsequent distillation, it seems preferable to employ sulphuric acid, as enjoined by Mr. Tennant. This excellent chemist also suggested the collection of the oxide that rises during the solution of the iridium ore. "As a certain quantity," he says, "of this oxide is extricated during the solution of the iridium in marine acid, that part may also be obtained by distillation†." M. Thenard does not seem to have been aware of this fact, since he lately ascribes to M. Laugier the merit of the same suggestion. "Il a reconnu que, dans le traitement de la mine de platine par l'acide nitro-muriatique, une partie d'osmium était attaquée, et que cette partie se vaporisait avec une certaine quantité d'acide‡."

It is hardly necessary to allude to the coarse method of the Marquis Ridolfi, described in the first volume of this *Journal*, page 259. To fuse crude platinum with half its weight of lead, to reduce the alloy to powder, to mix it with sulphur, to expose it to a strong heat in a covered crucible; to re-melt, with a little lead, the brittle button first obtained, and to hammer it at a white heat, upon a hot anvil, to extrude the lead, are directions nearly impracticable; and useless if they could be practised; for the foreign metals cannot be thus separated from the platinum.

The practice in Paris of alloying the pulverulent pure platinum with one eighth of its weight of arsenic; and of exposing the ingot of alloy to an open heat, progressively raised to whiteness, in order to expel the oxidized arsenic, is, I believe, generally abandoned. Uncertain portions of the arsenic used to lurk in the platinum, and to deteriorate its valuable qualities. The platinum of England and France seems to be at present equally pure, malleable, and ductile; and the price is nearly the same. There is, therefore, no ground for those jealous preferences which the narrow-minded of both nations are apt to give to their labours in this respect. As they form the most enlightened portion of Europe, so they should unite in the most cordial and generous co-operation, for the improvement of Science and the Arts. These reflections have been suggested by the following handsome remarks of M. Vauquelin, written during a period of national hostility :—" Though Dr. Wollaston operated only on 1000 grains of the ore of platinum, and of course had only six or seven grains of the new metals at his disposal, yet he determined their principal properties, which does infinite honour to his sagacity; for the thing appears at first view incredible. For my part, though I employed 60 marcs (about $39\frac{1}{3}$ troy pounds) of crude platinum, I found it very difficult to separate exactly the palladium and rhodium from the platinum and the other metals, which exist in that ore, and especially to obtain them in a state of purity*." Indeed, the most eminent men of the one nation are most ready to allow due merit to their rivals in the other. Jealous detraction is cultivated only by the subalterns, to lessen the sense of their own inferiority, or to gratify the prejudices of the great and little vulgar, whose suffrages they court.

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* *Phil. Tran.* 1805, page 326.

† *Phil. Tran.* 1804, page 416.

‡ *Traité de Chimie*, vol. ii, page 690, first edition.

ART. VII. *Contributions towards the Chemical Knowledge of Mineral Substances. By the late* MARTIN HENRY KLAPROTH.

[Continued from Page 40, of Vol. XII.]

Analysis of Cererit. (Cerite.)

I RECEIVED the specimens employed in the following Analysis from M. Geyer, of Stockholm, in the year 1788.

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* *Annales de Chimie*, for Nov. 1813.

A.

1. A piece of cererit was heated to redness in a platinum crucible; it lost 2 *per cent.* of its weight, and its original red colour became brown: its form remained unchanged.

2. 100 grains in fine powder, exposed for half an hour to a strong red heat, lost 5 grains, and acquired a darker colour.

B.

100 grains mixed with 200 of carbonate of potassa, and ignited in a platinum crucible, shewed no tendency to fusion, but assumed the appearance of a light brittle mass of an ash-grey colour. Being saturated and washed, the filtered alcaline liquor was colourless. It remained transparent when neutralized with nitric acid, and therefore contained no oxide of tungsten; nor was any other acid present, since the solution was not affected by the addition of solution of silver, mercury, lead, iron, baryta, *&c.*

The lixiviated residue was decomposed by repeated boiling in nitric acid. After the separation of the silica, the nitric solution was supersaturated with caustic potassa, boiled, filtered, neutralised with muriatic acid, and mixed with carbonate of potassa; but nothing was thrown down, nor did it become turbid.

C.

a. 400 grains of the levigated fossil were digested in 4 ounces of boiling muriatic acid, to which 1½ ounces of nitric acid were afterwards added. When every thing except the *silica* appeared to be dissolved, the liquid was filtered off, and the residue, washed and ignited, weighed 138 grains.

b. The excess of acid in the straw-coloured solution was then neutralized with liquid ammonia, taking care that nothing was thrown down; succinate of ammonia was then added, which caused a precipitate of succinate of iron, and which, after washing and ignition, afforded 14 grains of *oxide of iron.*

c. The solution, thus freed from iron, was precipitated by caustic ammonia; the *oxide of cerium* thus thrown down was edulcorated and ignited; it was of a brown colour, and weighed 218 grains.

d. The remaining fluid was boiled with carbonate of potassa: it afforded 9 grains of carbonate of lime, equal to 5 grains of pure *lime.* The fluid was then saturated with nitric acid, and tested by prussiate of potassa, which gave slight traces of the presence of copper. One hundred parts therefore of cererit contain,

Oxide of cerium	C *c*	54.50
Silica	— *a*	34.50
Oxide of iron	— *b*	3.50
Lime	— *d*	1.25
Water	A 2	5.
		98.75

Properties of the Oxide of Cerium.

a. Thrown down from its nitric solution by ammonia, oxide of cerium appears in the form of a muddy-red precipitate, which dries into a transparent and hard mass; when ignited, it acquires the form of a cinnamon-brown powder.

b. Carbonated alcalis separate a white carbonate, of which 100 grains (precipitated by carbonate of ammonia) lost 23 grains during solution in nitric acid; 100 grains of the same carbonate lost by ignition 35 grains; hence it is composed of

Oxide of cerium	65.
Carbonic acid	23.
Water	12.
	100

c. Before the blow-pipe, oxide of cerium becomes brilliantly luminous, without fusion. With phosphate of soda it produces a yellow bead which becomes colourless when cold. Borax produces the same appearance.

d. It undergoes no change by long-continued exposure to a bright red heat in a charcoal crucible.

e. Employed as an enamel, it gives a light-brown colour.

f. Neither caustic nor carbonated alcalis dissolve the recently precipitated oxide of cerium.

g. Carbonate of cerium is easily soluble in the acids, forming a sweetish astringent neutral solution; when diluted, the solutions are colourless, but become reddish-purple if concentrated.

h. Sulphate of cerium crystallizes in truncated octoëdra of a pale amethyst colour, and very difficultly soluble.

i. Nitrate of cerium is difficultly crystallizable and deliquescent.

k. Muriate of cerium forms prismatic crystals. Chlorine is evolved during the digestion of oxide of cerium in muriatic acid.

l. Acetic acid scarcely acts upon oxide of cerium, but it easily dissolves the carbonate, forming a very soluble white salt.

m. Sulphate of soda added to nitrate or muriate of cerium, forms a difficultly soluble white precipitate of subsulphate of cerium. This precipitate boiled in a solution of carbonate of soda, yields a perfectly pure carbonated oxide of cerium.

n. Sulphurous acid dissolves oxide of cerium, and the solution affords prismatic crystals of a pale amethyst colour.

o. The neutral phosphates, tartrates, and succinates form white precipitates in nitrate and muriate of cerium, which are soluble in acids.

p. Oxalic acid and the neutral oxalates occasion a precipitate in the solutions of cerium, which, unlike the former, is insoluble in nitric and muriatic acid.

q. Prussiate of potassa produces a white precipitate soluble in the acids.

r. Tincture of galls occasions no change.

s. Neither does sulphuretted hydrogen produce any change in the oxide of cerium; the precipitate occasioned in its solutions by hydro-sulphuret of ammonia, is of the same whitish-yellow colour as when pure ammonia is used.

Analysis of a compact Ore of Titanium from Arendal.

a. 300 grains of this ore in fine powder were fused with six times their weight of carbonate of potassa; when cold a grey porcellaneous mass was obtained, which by digestion in water afforded a carbonate of titanium, weighing, after having been washed and dried, 520 grains.

Prussiate of potassa and tincture of galls gave characteristic green and red precipitates in the muriatic solution of the above carbonate.

b. The alcaline washings of the carbonate did not become in the smallest degree turbid when neutralized by sulphuric acid.

This mineral therefore is to be regarded as pure oxide of titanium.

Analysis of the Topaz.

This mineral is especially characterized by the changes which it undergoes when subjected to a white heat; it then completely loses its original appearance, becomes soft, dull-white, and opaque, and *loses considerably in weight.*

[Here follows an account of the causes to which this loss of weight have been erroneously attributed, and to the experiments of Margraaf, Bergman, Wiegleb, Vauquelin, and Lowitz; the author also alludes to the probable existence of fluoric acid in the topaz, and then proceeds with his Analysis.]

Analysis of the Saxon Topaz. Sp. gr. 3.545.

A.

100 parts of coarsely-powdered topaz heated till the fragments began to fuse, in a coated retort, connected with the mercurio-pneumatic apparatus, afforded no gaseous matter, and lost little in weight.

The same portion, heated to whiteness in a blast-furnace for an hour, became white and pulverulent, and lost 22 per cent. in weight.

B.

300 grains of powdered topaz, distilled to dryness with sulphuric acid, produced a corrosion in the neck of the retort, resembling that of fluoric acid.

C.

200 grains of powdered topaz were projected into an ounce of nitre, fused in a platinum crucible; the mixture after a time concreted. The alcaline mass was then softened in water

saturated by nitric acid, and filtered. Lime-water, added to the filtered liquor, gave a precipitate having the characters of fluate of lime.

D.

100 parts of levigated topaz were fused in a platinum crucible, with 400 parts of nitrate of baryta; the fused mass was powdered, diffused through water, and mixed with excess of sulphuric acid; the precipitate being separated, excess of ammonia was added, and the liquor was filtered off and evaporated to dryness; the dry salt was wholly dissipated by heat, and gave no traces of a fixed alcali.

E.

a. 200 grains of topaz were bruised in a steel-mortar, and afterwards triturated as fine as possible in one of agate. The powder had sustained an increase of 4 grains. It was mixed in a silver crucible with 3 ounces of ley, (composed of equal parts of pure potassa and water,) evaporated to dryness, and ignited for an hour. The grey mass was then softened with water, and digested in muriatic acid, which entirely dissolved it, forming a clear yellow liquor. This was evaporated to dryness, water was poured upon it, and the insoluble siliceous residue thus obtained being completely washed, dried, and ignited, weighed 74 grains; deducting 4 grains derived from the mortar, the quantity of *silica* in 100 of the topaz is thus found to amount to 35 grains.

b. The muriatic solution, heated to its boiling point, was saturated by carbonate of potassa; the edulcorated precipitate weighed 218 grains.

c. 109 grains of this precipitate were re-dissolved in muriatic acid, and the solution tested by oxalate of potassa, which occasioned no change. It was then precipitated by caustic potassa, which being added in excess re-dissolved the whole of the precipitate by the aid of heat, with the exception of a scarcely perceptible portion of *oxide of iron*. The alcaline liquor, supersaturated by muriatic acid, was mixed with carbonate of potassa, and the precipitate thus obtained being dissolved in sulphuric acid, and mixed with acetate of potassa, afforded nothing but crystals of alum.

d. The other half of the precipitate *b* was digested in distilled vinegar; the solution saturated by carbonate of ammonia afforded a precipitate which, after due elutriation and ignition, weighed 59 grains, and had the proporties of pure *alumine.*

e. The liquid remaining, after the separation of the precipitate *b*, was evaporated to a small bulk, rendered neutral by muriatic acid, and mixed with lime-water: a precipitate was thus formed, having the properties of *fluate of lime*; acted upon by sulphuric acid, it evolved glass-corroding fumes.

The above experiments sufficiently demonstrate the existence of *fluoric acid* in the topaz, without accurately determining its relative proportion. Experience leads me to believe, that the loss in the earthy constituents does not exceed 1 per cent.; hence the deficiency of 5 per cent. may be referred to fluoric acid, and the components of the pale yellow Saxon topaz may be stated as follows:

Silica	*a*	35
Alumina	*d*	59
Fluoric acid	*e*	5
Oxide of iron.	*c*	a trace.
Loss		1
		100

Analysis of Zoisit, from Carinthia.

The specific gravity of the crystals of this mineral, used in my analysis, was =3.315.

a. 100 grains in fine powder were mixed with a solution of 250 grains of soda, and evaporated and fused in a silver crucible; the mass was softened with water, and dissolved in excess of muriatic acid, with which it formed a clear yellow solution; this was again evaporated to dryness, and the residue, treated by very dilute muriatic acid, left 45 grains of *silica.*

b. The liquid was mixed with caustic ammonia, and the precipitate separated by filtration; carbonate of soda was then added, by which 33.5 grains of carbonate of lime were thrown down equivalent to 18.5 grains of *lime.*

c. The precipitate thrown down by caustic ammonia was boiled in caustic potassa, which dissolved it, with the exception of some brown residuary matter. This residue, dissolved in nitric acid, and precipitated by ammonia, afforded 3 grains of oxide of iron; carbonate of ammonia, subsequently added to the washings, furnished an additional precipitate of carbonate of lime, equal to 2.5 of *lime.*

d. To obtain the ingredient dissolved by the caustic potassa, the solution was slightly supersaturated by sulphuric acid, and precipitated by carbonate of potassa; the precipitate was washed, and boiled in distilled vinegar, which being again saturated with ammonia, and the precipitate washed, dried and ignited, gave 29 grains of pure *alumina.*

Hence 100 parts of crystallized zoisit consist of

Silica	*a*		45
Alumina	*d*		29
Lime	*b*	18.50	21
——	*c*	2.50	
Oxide of iron	c		3
			98

Analysis of the foliated Augite of Carinthia.

A.

This mineral is difficultly fusible in small fragments; it runs before the blow-pipe into an opaque olive-coloured slag. Its powder is greenish grey; heated to redness, it becomes pale-brown, without loss of weight,

B.

a. 100 grains were mixed with a solution containing 200 of caustic soda, evaporated to dryness, and ignited. The resulting mass tinged the water which was affused of a pale-green; dissolved in muriatic acid, evaporated to dryness, and again treated with very dilute muriatic acid, *silica* remained, which after being heated red-hot, weighed 52.5 grains.

b. The muriatic solution was saturated by caustic ammonia, which caused a bulky brown precipitate; this being separated by filtration, a clear colourless solution was obtained, which, by the addition of carbonate of soda, yielded 16.5 grains of carbonate of lime, =9 grains of *lime.*

c. The precipitate by ammonia was transferred while yet moist, into a boiling solution of caustic potassa; a portion was dissolved, and the remainder collected upon a filter. Muriate of ammonia was added to the alcaline solution, which threw down 7.25 grains of *alumina.*

d. The remaining portion was dissolved in dilute nitro-muriatic acid; to this solution carbonate of soda was added, to throw down oxide of iron, amounting to 16.25 grains.

e. The residuary liquid was boiled, and completely decomposed, with excess of carbonate of soda; carbonate of magnesia was thus thrown down, which yielded, on ignition, 12.5 grains of *magnesia.*

C.

100 grains of levigated augite were fused with 500 grains of nitrate of baryta; the resulting mass was powdered, mixed with water, neutralized by sulphuric acid, and the precipitate thus formed separated upon a filter. The clear liquid was precipitated by ammonia, the precipitate removed, evaporated to dryness, and the dry salt ignited; as this contained sulphate of magnesia, it was again dissolved and decomposed by acetate of baryta; the filtered liquid was evaporated, and the residue ignited; there remained a blackish matter, weighing 1.25 grains, which was washed, to separate the charcoal resulting from the combustion of the acetic acid; the washings afforded a portion of carbonate of potassa, equivalent to about half a grain of *potassa.*

100 parts, therefore, of this lamellar augite consist of

Silica	B *a*	52.50
Magnesia	— *e*	12.50
Lime	— *b*	9
Alumine	— *c*	7.25
Oxide of iron	— *d*	16.25
Potassa	C	0.50
		98

Analysis of Conchoidal Apatite, from Zillerthahl.

The specific gravity of this mineral is 3.190. After ignition it becomes colourless, without losing its transparency, or decrepitating, neither does it lose perceptibly in weight. Sprinkled upon hot coals, it exhibits no phosphorescence.

Having, by previous trials, ascertained the presence of phosphoric acid and lime, its decomposition was effected as follows :

a. 100 grains in powder were digested in muriatic acid, which soon formed a solution, a few particles of talc only remaining, and weighing 0.75 grain. This loss on the hundred parts was made good by the addition of a similar weight of the pure mineral.

b. The muriatic solution was nearly saturated with caustic ammonia, solution of oxalic acid was then added as long as it formed a precipitate of oxalate of lime, which was collected, washed, dried, and strongly heated in a platinum crucible ; it was then dissolved in muriatic acid, and thrown down by carbonate of soda, in the state of carbonate of lime, which weighed, when washed and dried, 91 grains, =50 grains of pure *lime.*

c. To separate the phosphoric acid from the liquid, from which the lime had been abstracted by oxalic acid ; it was evaporated to dryness, and the saline residue gradually ignited in a platinum crucible, during which operation muriate of ammonia passed off in white fumes. The remainder, when cold, appeared as a vitreous mass, blackened by a portion of charcoal derived from the decomposition of the oxalic acid. It weighed 47.5 grains. It was dissolved in boiling water, by which 1 grain of finely-divided charcoal was separated.

d. The acid liquid was neutralized with ammonia, which produced some turbidness, and a precipitate, weighing 7 grains, and composed of phosphate of lime, was deposited; it fused before the blow-pipe into a porcellaneous bead.

Deducting these 7 grains, and the 1 grain of coal, the weight of the phosphoric acid is 39.5. If we consider the 7 grains of undecomposed phosphate as composed of 3.75 of lime, and 3.25 of phosphoric acid, the components of the apatite obtained in this analysis are,

Lime	*b* 50	53.75
	d 3.75	
Phosphoric acid	*d* 39.50	42.75
	3.25	
		96.50

But a small portion of phosphoric acid was probably volatilized by heat, so that the loss, amounting to 3.5 grains, may be attributed to that source, and the components of this mineral will then stand thus:

Lime	53.75
Phosphoric acid	46.25
	100

A trace of iron and of manganese was also discovered in this apatite, but in quantity too small to be appreciated by weight.

The analysis of the Saxon apatite agrees with the above results: it consists of

Lime	55
Phosphoric acid	45*

Vauquelin's analysis of the chrysolite, or asparagus stone of Spain, also afforded nearly the same proportions, namely,

Lime	54.28
Phosphoric acid	46.72†

Analysis of the Columnar Brown-spar of Mexico.

This mineral is among the treasures imported from America by M. Von Humboldt.

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* *Berg. Journ.* 1788. 1 Band. S. 296.

† *Jour. des Mines*, xxxvii. p. 26.

A.

100 parts were heated in a covered crucible; it decrepitated into a coarse powder, losing 5 per cent. of weight, but remaining white. The heat was increased to redness, when it lost 11 per cent., and became of a blackish-grey colour.

B

Digested in cold and moderately strong nitric acid, it dissolved slowly, and with sparing effervescence, forming a transparent straw-coloured solution. If the acid be heated, it effervesces strongly forming a reddish-brown solution, and the undissolved portion of the fossil appears orange-yellow; when perfectly dissolved, the colour of the solution is pale reddish brown.

C.

100 grains of the coarsely-powdered mineral were dissolved in nitric acid, and the solution neutralized by caustic ammonia. A brown flocculent precipitate fell, weighing, when dry, 14 grains; this was re-dissolved in nitro-muriatic acid, the excess of acid was then neutralized by soda, and the iron separated by succinate of ammonia. The succinate of iron yielded, after ignition, 5.5 grains of magnetic oxide of iron. But, since the iron exists in this mineral in the state of carbonate, the above result must be assumed to indicate 7.5 grains of *carbonate of iron.* The liquid from which the iron had been thrown down was heated to its boiling point, and mixed with carbonate of soda, which gave a white precipitate, becoming black when heated, and weighing, after ignition, 4 grains. Digested in nitric acid, the greater portion of this precipitate was dissolved, but there remained a black residue, consisting of oxide of manganese, and weighing 1.25 grains, which I consider as equal to 2 grains of *carbonate of manganese.* Carbonate of soda threw down from the remaining nitric solution 3.5 grains of *carbonate of magnesia*

b. The excess of ammonia, in the original solution *a*, was supersaturated by nitric acid, and carbonate of soda added to the cold solution as long as it occasioned a precipitate, which, collected, edulcorated, and dried, weighed 49 grains, and consisted of *carbonate of lime.*

c. When the residuary liquor was boiled, it became turbid, and a white precipitate fell, the separation of which was completed by the addition of a little soda. Washed and dried, it weighed 31 grains. Dissolved in sulphuric acid and evaporated, it furnished crystals of sulphate of magnesia, which being re-dissolved, left 4 grains of sulphate of lime, =2.5 grains of *carbonate of lime*; this being deducted, there remained 28.5 grains of *carbonate of magnesia.*

100 parts, therefore, of this mineral consist of,

Carbonate of lime......	C	*b* 49	51.50
		c 2.50	
Carbonate of magnesia ..		*a* 3.50	32
		c 28.50	
Carbonate of iron......		*a*	7.50
Carbonate of manganese		*a*	2
Water................	A	......	5
			98

Analysis of Dolomite.

[In these analyses, Klaproth dissolved the mineral in dilute nitric acid, and saturated with caustic ammonia, which threw down oxide of iron, when present; he then added carbonate of ammonia, by which carbonate of lime was precipitated; and lastly the magnesia was separated, either by boiling with carbonate of potassa; or by oxalate of potassa. The following are the results.]

	Compact Dolomite from St. Gothard.	Decomposing Dolomite from the Apennines.	Compact Dolomite from ditto.	Antique Dolomite from Tenedos.
Carbonate of Lime . . .	52	59	65	51.50
Carbonate of Magnesia .	46.50	40.50	35	48.
Oxide of Iron and of Manganese	0.75			
Loss	0.75	0.50		0.50

Analysis of Anhydrite.

A.

A fragment of blue anhydrite (from Sulz on the Neckar) weighing 300 grains, was kept red hot in a platinum crucible for half an hour; it became soft and yellowish-white, but lost no weight.

B.

Thirty grains of the powdered mineral were boiled in 12 ounces of water; 8 grains were dissolved, but the solution did not render nitrate of silver in the least turbid, so that it contained no muriatic acid.

C.

a. 200 grains of pulverized anhydrite were boiled with 400 grains of carbonate of potassa in 8 ounces of water for a quarter of an hour. The alcaline liquor was then filtered off, neutralized by muriatic acid, and mixed with muriate of baryta as long as a precipitate was produced. The dry sulphate of baryta weighed 345 grains = 114 grains of dry *sulphuric acid.*

b. The residue upon the filtre dissolved with effervescence in dilute nitric acid, excepting 0.5 grain of *silica*; supersaturated with caustic ammonia the solution gave no precipitate; it was therefore again rendered neutral by nitric acid, and tested with prussiate of potassa, and after a while it deposited a small portion of prussian blue indicating not more than 0.20 grain of *oxide of iron.*

c. This being separated the solution was saturated whilst boiling hot, with carbonate of potassa; carbonate of lime was thus thrown down, weighing when washed and dried in a gentle heat, 153 grains = 84 grains of *lime.* This carbonate was then neutralized with sulphuric acid, evaporated to dryness, and ignited; it thus afforded 198 grains of regenerated sulphate of lime.

100 parts therefore of this blue anhydrite were resolved into

Lime	42.
Sulphuric acid	57.
Oxide of iron	0.10
Silica (probably adventitious) . .	0.25
	99.35

[Another specimen of anhydrite from Hall in the Tyrol, contained

Lime	41.75
Sulphuric acid	55.
Muriate of soda	1.
	97.75

The source of loss in this analysis is not adverted to. The composition of sulphate of lime, deduced fron the mean of Klaproth's *Analyses of the Anhydrite,* agrees exactly with the equivalents upon Dr. Wollaston's scale.]

Analysis of Bitter Spar from Hall in the Tyrol.

This spar accompanies the anhydrite described in the last paragraph. Its component parts are

Carbonate of lime	68.
Carbonate of magnesia	25.50
Carbonate of iron	1.
Water	2.
(Adhering clay)	2.
	98.50

Analysis of the Green Earth of Verona.

This substance, found at Monte Baldo in the Veronese, is much esteemed as a green pigment. After having been heated red hot it acquires a brown colour, and additional hardness, losing 6 per cent. of its weight. It is not soluble in acids, nor do they alter its colour.

A.

a. 100 grains were mixed with 200 of caustic potassa dissolved in water, and the mixture evaporated and ignited. In this case the colour remained unchanged till the mass became red hot, when it grew yellowish brown: it was dissolved in muriatic acid; the solution was evaporated to dryness, and the residue being again digested in very dilute muriatic acid, 53 grains of *silica* were obtained.

b. Carbonate of soda was then added to the remaining cold liquid so as to saturate it, which threw down *oxide of iron*, weighing when washed and ignited, 28 grains.

c. After the separation of oxide of iron the residuary liquid was made boiling hot, and carbonate of soda added in excess, which threw down *magnesia* weighing after ignition 2 grains.

B.

One hundred grains were fused with 500 of nitrate of baryta, and the residuary mass supersaturated with dilute sulphuric acid. The precipitate being separated, the clear liquor was mixed with excess of carbonate of ammonia, filtered, evaporated to dryness, and the residuum ignited in a platinum crucible. After the escape of the sulphate of ammonia, there remained 19 grains of sulphate of potassa, equal to 10 grains of *potassa*.

The components therefore of this mineral are as follow:

Silica	53
Oxide of iron	28
Magnesia	2
Potassa	10
Water	6
	99

Analysis of the Alum Stone of Tolfa.

As this mineral affords alum by lixiviation, sulphuric acid, alumina, and potassa, were to be looked for among its components.

A.

One hundred grains were strongly heated in a coated glass retort; sulphurous acid, water, and sulphuric acid passed over, but there were no traces of uncombined sulphur. The loss of weight amounted to 29 grains.

B.

Two hundred grains were gently heated so as to drive off *water* only: the loss amounted to 6 grains.

C.

a. 200 grains in fine powder were fused with twice their weight of dry carbonate of soda; the cold mass was powdered, dissolved in dilute muriatic acid, evaporated to dryness, redissolved in water, and filtered; there remained upon the filter 113 grains of *silica.*

b. The muriatic solution was divided into two equal portions, one of which was mixed with solution of muriate of baryta; 50 grains of sulphate of baryta = 16.5 dry *sulphuric acid* were obtained.

c. The other half of the solution was precipitated by ammonia. The precipitate consisted of *alumina*, which when purified, edulcorated and ignited weighed 19 grains.

D.

One hundred grains were fused with 300 of nitrate of baryta; the resulting mass was softened with water, mixed with excess of sulphuric acid, evaporated nearly to dryness, and then boiled in water and filtered; the clear liquid was neutralized by ammonia, and after the separation of the precipitate was evaporated to dryness and ignited, there remained 7 grains of sulphate of potassa = 4 of pure *potassa.*

The components therefore are

Silica	C *a*	56.50
Alumina	c	19.
Sulphuric acid	*b*	16.50
Potassa	D	4.
Water	B	3.
		99.

Analysis of the Alum Slate of Freienwald.

A.

a. 1000 grains were boiled for half an hour with 20 ounces of distilled water; then filtered and the residue edulcorated. The filtered liquid was colourless, did not change vegetable blues, but had a slightly vitriolic taste.

b. Half of this solution was mixed with muriate of baryta by which 23 grains of sulphate of baryta were separated; and afterwards, prussiate of ammonia threw down 40 grains of prussiate of iron.

c. The other half was mixed with oxalate of potassa; it became pale yellow and moderately turbid, the colour probably resulting from a little oxalate of iron: the mixture became slowly clear, and the precipitate being collected weighed after ignition 2.5 grains, and had the properties of lime slightly contaminated by oxide of iron.

The matter therefore, soluble in water, consists of sulphate of lime and sulphate of iron, the relative proportions of which may be estimated as follows: 1000 parts of the ore gave 46 of sulphate of baryta = 15.18 dry sulphuric acid, of which 7 parts saturated the 5 of lime, producing 15 parts of crystallized gypsum The remaining 8.18 of acid combined with 8.5 of oxide of iron would produce 18 parts of effloresced sulphate of iron.

B.

Two hundred grains of the ore were boiled in water with 400 of dry carbonate of soda; the filtered liquid was of a dark brown colour; neutralized with muriatic acid, it afforded no trace of sulphuretted hydrogen: this neutral solution gradually deposited a bulky dark brown matter which, collected and dried, weighed 12 grains, and which, heated in a platinum crucible, burned away without any odour of sulphur, and left 1 grain of white alumina.

C.

Two hundred grains were digested in muriatic acid, during which not the smallest trace of sulphuretted hydrogen was perceived, nor was there much action; but during the gradual addition of nitric acid, nitrous gas was evolved and the black colour of the ore became brown; the filtered solution was yellow and mixed with muriate of baryta furnished 54 grains of sulphate of baryta (after ignition).

D.

a. 1000 grains of the ore recently from the mine, were heated in a retort, the recipient being connected with the pneumatic apparatus. 220 cubic inches of gas were evolved, having the properties of a mixture of sulphuretted and carburetted hydrogen.

b. The liquid produced weighed 133 grains; it was yellow and smelt of hydro-sulphuret of ammonia; it changed reddened litmus paper to blue, and produced fumes when brought to a glass rod dipped in muriatic acid; it occasioned a brown precipitate in solution of lead; became milky when neutralized by muriatic acid; filtered and evaporated, it left 2 grains of muriate of ammonia.

c. The black residue in the retort weighed 750 grains; its charcoal was burned off and there remained 660 grains of a reddish-brown matter, so that the *carbonaceous loss* was 90 grains.

d. 132 grains of this brown residuum, (one fifth,) mixed with twice its weight of caustic soda and fused, furnished a greenish-brown mass which, digested in water gave a pale green solution; supersaturated by muriatic acid, evaporated, and again digested with water acidulated by the same acid, *silica* remained, weighing, after ignition, 80 grains.

e. The remaining liquid was saturated with carbonate of potassa, and the washed precipitate thus obtained boiled in liquid caustic potassa to abstract the *alumina*, which, thrown down by muriate of ammonia, washed and heated, weighed 32 grains.

f. The brown residue, insoluble in the caustic alcaline solution, was dissolved in dilute sulphuric acid, and evaporated to dryness; during the evaporation *sulphate of lime* separated, which was carefully collected; it weighed rather more than 2 grains: the dry mass was ignited and washed, and the *oxide of iron*, which then remained, being collected and mixed with oil and ignited became magnetic, and weighed 14.5 grains. The washings saturated, when boiling hot, with carbonate of potassa, yielded a slight trace of carbonate of magnesia.

E.

a. 200 grains of the ore were slowly distilled in a sand-heat, so that no gas was evolved; 21.5 grains of water very slightly sulphurous, passed into the receiver, and an extremely thin coating of sulphur was remarked in the neck of the retort.

b. The dried ore was taken out of the retort and heated upon a tile; it exhaled a slight smell of sulphur, without either smoke or flame. The loss during the combustion amounted to 45 grains which may be estimated as charcoal and sulphur with perhaps a little water.

c. The residuum was mixed with 200 grains of sulphuric acid and 400 of water, evaporated, and strongly ignited for half an hour: it was then washed in water, and the filtered washings were saturated with ammonia which threw down half a grain of magnesia.

d. The liquid was then evaporated to dryness, and the saline mass heated till it no longer evolved fumes; the dry salt weighed 4.5 grains, and had the properties of a mixture of sulphate and muriate of potassa, the sulphate being to the muriate as 2 to 1. [It is here not very evident how the muriate of potassa escaped the action of the sulphuric acid, during the ignition in *c.*]

F.

It appears from these experiments,

1. That there is no bitumen, but only charcoal in this alum ore, since neither bituminous oil, nor smoke, nor flame, are produced during its distillation and combustion.

2. That the sulphur is not combined with iron in the state of pyrites, but in some peculiar combination with the carbonaceous matter; no pyrites is visible in the ore, and alcalis do not extract it, nor does muriatic acid evolve sulphuretted hydrogen.

G.

The estimation of the relative weights of the constituents of this ore is therefore attended with difficulty; the ingredients that distinguish it as an alum ore, are alumina and sulphur. Direct experiment gives the proportion of the former =160 in 1000. The proportion of sulphur may be deduced from the result of process C, which affords 270 grains of sulphate of baryta from one thousand of the ore. But of these 270 grains, 46 may be ascribed to the vitriol and gypsum, and 20 to the sulphate of potassa (supposing 15 parts of that salt to be contained in 1000 of the ore). We must therefore subtract 66 from 270, and there remain 204 parts of sulphate of baryta, as the equivalent of the sulphur of the ore. These 204 parts are equal to 90.75 of sulphuric acid, (specific gravity 1.850) or =28.5 of sulphur, which being deducted from the joint amount of the sulphur and charcoal, amounting to 225 grains (E*b*) leave 196.5 grains for the weight of the charcoal.

H.

Assuming that 1000 parts of crystallized alum yield upon average, when decomposed by muriate of baryta, 945 grains of sulphate of baryta, it follows that 1000 parts of the alum ore of Freienwald, containing 28.5 of sulphur, should produce sulphuric acid sufficient (with the due addition of potassa) to form 260 parts of alum, for which scarcely one-sixth part of the alumine in the ore is required. But the quantity of alum produced at the works, falls infinitely short of this estimate in consequence of the very imperfect acidification of the sulphur.

I.

The following view of the components of this alum ore is suggested by the above experiments.

Sulphur	G		28.50
Carbon	G		196.50
Alumina	D *e*		160.
Silica	D *d*		400.
Black oxide of iron (with a very slight trace of manganese	D *f*	72.5	64.
Of which deduct for the composition of sulphate of iron		8.5	
Sulphate of iron	A *c*		18.
Sulphate of lime	A *c*		15.
Magnesia	E *e*		2.50
Sulphate of potassa	E *d*		15.
Muriate of potassa	E *d*		5.
Water	E *a*		107.50
			1012.

Some of the above results, however, require further investigation, and the magnesia is probably in the state of sulphate in the ore. The excess of 1.5 per cent. in the analysis is insignificant in such experiments.

ART. VIII. *Description of an Electro-magnetical Apparatus for the Exhibition of Rotatory Motion. By* M. FARADAY, *Chem. Assist. in the Royal Institution.*

THE account given in the *Miscellanea* of the last Journal, of the apparatus invented in illustration of the paper in the body of that Number, being short and imperfect; a plate is given in the present Number, presenting a section of that apparatus, and a view of a smaller apparatus, illustrative of the motions of the wire and the pole round each other. The larger apparatus is delineated, Fig. 1. Plate VII. on a scale of one-half. It consists of two glass vessels, placed side by side with their appendages. In that on the left of the plate the motion of a magnetic pole round the connecting wire of the voltaic battery is produced. That a current of voltaic electricity may be established through this cup, a hole is drilled at the bottom, and into this a copper pin is ground tight, which projects upwards a little way into the cup, and below is rivetted to a small round plate of copper, forming part of the foot of the vessel. A similar plate of copper is fixed to the turned wooden base on which the cup is intended to stand, and a piece of strong copper wire, which is attached to it beneath, after proceeding downwards a little way, turns horizontally to the left hand, and forms one of the connexions. The surfaces of these two plates intended to come together, are tinned and amalgamated, that they may remain longer clean and bright, and afford better contact. A small cylindrical and powerful magnet has one of its poles fastened to a piece of thread, which, at the other end, is attached to the copper pin at the bottom of the cup; and the height of the magnet and length of the thread is so adjusted, that when the cup is nearly filled with clean mercury, the free pole shall float almost upright on its surface.

A small brass pillar rises from the stand behind the glass-vessels: an arm comes forward from the top of it, supporting at its extremity a cross wire, which at the place on the left hand, where it is perpendicularly over the cup just described, bends downwards, and is continued till it just dips into the centre of the mercurial surface. The wire is diminished in size for a short distance above the surface of the mercury, and its lower extremity amalgamated, for the purpose of ensuring good contact; and so also is the copper pin at the bottom of the cup. When the poles of a voltaic apparatus are connected with the brass pillar, and with the lateral copper wire, the upper pole of the magnet immediately rotates round the wire which dips into the mercury; and in one direction or the other, according as the connexions are made.

The other vessel is of the form delineated in the plate. The stem is hollow and tubular; but, instead of being filled by a plug, as is the aperture in the other vessel, a small copper socket is placed in it, and retained there by being fastened to a circular plate below, which is cemented to the glass foot, so that no mercury shall pass out by it. This plate is tinned and amalgamated on its lower surface, and stands on another plate and wire, just as in the former instance. A small circular bar magnet is placed in the socket, at any convenient height, and then mercury poured in until it rises so high that nothing but the projecting pole of the magnet is left above its surface at the centre. The forms and relative positions of the magnet, socket, plate, &c. are seen in fig. 2.

The cross wire supported by the brass pillar is also prolonged on the right hand, until over the centre of the vessel just described; it then turns downwards, and descends about half an inch: it has its lower extremity hollowed out into a cup, the inner surface of which is well amalgamated. A smaller piece of copper wire has a spherical head fixed on to it, of such a size that it may play in the cup in the manner of a ball and socket-joint, and being well amalgamated, it, when in the cup, retains sufficient fluid mercury by capillary attraction to form an excellent contact with freedom of motion. The ball is prevented from falling out of the socket by a piece of fine thread, which, being fastened to it at the top, passes through a small hole at the summit of the cup, and is made fast on the outside of the thick wire. This is more minutely explained by Fig. 3, and 4. The small wire is of such a length that it may dip a little way into the mercury, and its lower end is amalgamated. When the connexions are so made with the pillar and right hand wire, that the current of electricity shall pass through this moveable wire, it immediately revolves round the pole of the magnet, in a direction dependant on the pole used, and the manner in which the connexions are made.

Fig. 5, is the delineation of a small apparatus, the wire in which revolves rapidly, with very little voltaic power. It consists of a piece of glass tube, the bottom part of which is closed by a cork, through which a small piece of soft iron wire passes, so as to project above and below the cork. A little mercury is then poured in, to form a channel between the iron wire and the glass tube. The upper orifice is also closed by a cork, through which a piece of platinum wire passes, which is terminated within by a loop; another piece of wire hangs from this by a loop, and its lower end, which dips a very little way into the mercury, being amalgamated, it is preserved from adhering either to the iron wire or the glass. When a very minute voltaic combination is connected with the upper and lower ends of this apparatus, and the pole of a magnet is placed in contact with the external end of the iron wire, the moveable wire within rapidly rotates round the magnet thus formed at the moment; and by changing either the connexion, or the pole of the magnet in contact with the iron, the direction of the motion itself is changed.

The small apparatus in the plate is not drawn to any scale. It has been made so small as to produce rapid revolutions, by the action of two plates of zinc and copper, containing not more than a square inch of surface each.

In place of the ball and socket-joint, (fig. 3, and 4,) loops may be used; or the fixed wire may terminate in a small cup containing mercury, with its aperture upwards, and the moveable wire may be bent into the form of a hook, of which the extremity should be sharpened, and rest in the mercury on the bottom of the cup.

Art. IX. *On the Atomic Weight of Silver, and on the Constitution of Liquid Muriatic Acid at different Densities; by* Andrew Ure, M.D., F.R.S., *Professor of the Andersonian Institution at Glasgow,* M. G. S., *&c.*

[This paper forms the introduction to an elaborate Memoir on the nature and manufacture of Chloride (Oxymuriate) of Lime, and on the atomic weights of Silver and Manganese, with which we have been favoured by Dr. Ure, and which will appear in our next Number.]

Upwards of four years ago, I published an account of a set of experiments, which I had made on the saturating power of liquid muriatic acid of 1.102, with a table of its progressive densities, and corresponding acid strengths. This table was favourably received by the chemical world, and has since been adopted into most of our respectable treatises on Chemistry. Having had occasion last spring to subject muriatic acid in different states of dilution, to a very rigorous examination, I perceived small deviations in the new results, from my former tabular quantities, which induced me to revise the whole with the greatest possible care *. My first business, however, was to settle the prime equivalent or atomic weight of silver. For this purpose I revived from the washed chloride of this metal a portion of silver which I re-dissolved in pure nitric acid, filtered the solution, re-produced the chloride, and once more revived the silver by ignition with pure carbonate of potash. The metal being well boiled in distilled water, washed, and dried, I regarded as perfectly pure, and dissolved a known weight of it in a sufficient quantity of nitric acid. The solution was then diluted with water, so that the silver formed one-twentieth of the whole. I next dissolved, in some of the same double distilled water, 75 grains of pure sal gem, (native muriate of soda, which stood the tests of muriate of barytes and oxalate, and phosphate of ammonia,) and mixed with that solution one of the nitrate of silver, containing 137.5 grains of metal. Gentle agitation was employed during the mixture. In 24 hours the supernatant liquid, having become quite limpid, was tested, and found to contain redundant muriate. Small quantities of a very dilute solution of nitrate of silver were progressively added with much agitation, till the clear supernatant liquid was found free from every vestige of chlorine. Nor did muriate of soda shew in it any trace of silver. The total quantity of silver expended to produce this effect was 138.75 grains. This experiment was repeated on different quantities, and with a variation in the mode of mixture, but still the same proportion obtained in the result. From the quantity of salt and of silver in their respective solutions, and from the delicacy of my balance, I could add either ingredient to the mixture within $\frac{1}{1000}$ of a grain. In three experiments, however, in which I used sea salt, recrystallized with care in regular cubes, I obtained anomalous results, in consequence of minute portions of adhering muriates of potash or lime. From such salt the prime equivalent of silver appeared to be at different times 13.73, 13.75, and 13.7. It is the tendency of the slightest admixture of these salts, or of sulphate of soda, to lower the apparent atomic weight of silver. If therefore the prime equivalent of chloride of sodium be called 4.5 chlorine $+3$ sodium $=7.5$, then we must infer from the above experiments that the prime equivalent of silver is 13.875; or, taking the proportional weight of hydrogen in water as the radix of the equivalent scale, chloride of sodium will become 60, and silver 111: or, in volume on Sir H. Davy's plan, 120 and 222.

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* Subsequent researches have not enabled me to make any improvement on my tables of Sulphuric and Nitric Acids. I believe them to be very near the truth.

The same solution of silver was used for determining the quantity of chlorine in liquid muriatic acid, having found by experience that its saturation by carbonate of potash, soda, or lime, and evaporation of the chlorides, did not afford results of the minute and consistent accuracy, required by the theory of equivalents. Liquid acid having the specific gravity of 1.200, was diluted with 9 times its weight of water, when its density at 60° Fahr. became 1.020. On this dilute acid I made many experiments with the nitrate of silver, and satisfied myself at last that 1000 grains of it were equivalent to 122⅓ grains of silver, which, from the above-recited experiments, represent 39.675 of chlorine; the quantity, therefore, present in 100 grs. of acid, at the density of 1.200. If to 39.675 we add one prime equivalent of hydrogen $= \frac{39.675}{36} = 1.1021$, the sum 40.7771 will represent the corresponding quantity of muriatic acid gas. Hence, for ordinary practice, if we estimate in round numbers, the value of liquid acid, specific gravity 1.20, at 40 *per cent.* of chlorine and 41 of muriatic gas, we shall commit no error of consequence.

The correspondence between the above experimental results, and those given by Sir H. Davy in his valuable *Elements of Chemistry*, is almost exact, though the two methods are sufficiently distinct. Sir H. and Mr. E. Davy found that 47.25 grains of water at 43° Fahr. barom., 30.2 absorbed 34.8 grains of gas, and formed a solution of specific gravity 1.21; and the whole precipitated by nitrate of silver, afforded about 132 grs. of dry horn silver. Again 57.5 grains of water, at 44°, barom. being 30.1, gained nearly 38 grains by absorbing acid gas, and formed a solution of specific gravity 1.2*. The only other point which they determined by experiment was for the specific gravity 1.114. Let us compare a little their direct experiment of condensing the gas in water (whose result agrees very nearly with mine,) with their result from chloride of silver. If 82.05 grains of acid of 1.21 resulted from the combination of 47.25 water + 34.8 acid gas, then 100 of such acid, at 43°, should contain just 42.41 parts, as given in their table. But if we calculate the acid gas present in the same 82.05 grains of liquid acid, from the weight of horn silver which they obtained, we shall have a considerably different result. Naming the equivalent of chloride of silver, 18.375 (4.5 + 13.875) we shall have this proportion; 18.375 : 4.5 :: 132 : 32.7 of chlorine, corresponding to 33.225 of acid gas, instead of 34.8, as by their direct experiment. If, with Dr. Thomson, we call chloride of silver 18.25 (13.75 + 4.5), then we have the proportion 18.25 : 4.5 :: 132 : 32.548 of chlorine, or 33.452 of muriatic gas. Finally, by Dr. Wollaston's scale, 132 grains of muriate of silver are equivalent to 33.5 of the acid gas. The first number makes the quantity of muriatic gas, in 100 parts of acid of 1.21, to be only 40.5; the second, 40.77; the third, 40.83, while the number in their table is 42.43. In like manner, when I attempted to get very nice results by weighing the chloride of silver, I found a perceptible and fluctuating deficiency.

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* *Elements*, p. 252, Note.

From the excellent accordance which exists between Sir H. and Mr. E. Davy's experiments on the direct combination of the acid gas and water, and my result drawn from the silver equivalent, I shall consider that 100 grains of acid, specific gravity 1.20, contain 39.675 of chlorine. *That* acid was mixed with distilled water in successive proportions, and the mixtures being placed in well-stopped phials, and agitated, were left to cool to 60° Fahr. Their specific gravities were then taken with every requisite care.

The following table is the result of these experiments. The densities marked with an asterisk are experimental, the rest are interpolated; but the latter are, I believe, as well as the former, entitled to the entire confidence of practical chemists.

Table of Muriatic Acid.

Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.	Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.
100*	1.2000	39.675	40.777	81	1.1641	32.136	33.029
99	1.1982	39.278	40.369	80*	1.1620	31.746	32.621
98	1.1964	38.882	39.961	79	1.1599	31.343	32.213
97	1.1946	38.485	39.554	78	1.1578	30.946	31.805
96	1.1928	38.089	39.146	77	1.1557	30.550	31.398
95*	1.1910	37.692	38.738	76	1.1536	30.153	30.990
94	1.1893	37.296	38.330	75	1.1515	29.757	30.582
93	1.1875	36.900	37.923	74	1.1494	29.361	30.174
92	1.1857	36.503	37.516	73	1.1473	28.964	29.767
91	1.1846	36.107	37.108	72	1.1452	28.567	29.359
90*	1.1822	35.707	36.700	71	1.1431	28.171	28.951
89	1.1802	35.310	36.292	70*	1.1410	27.772	28.544
88	1.1782	34.913	35.884	69	1.1389	27.376	28.136
87	1.1762	34.517	35.476	68	1.1369	26.979	27.728
86	1.1741	34.121	35.068	67	1.1349	26.583	27.321
85	1.1721	33.724	34.660	66	1.1328	26.186	26.913
84	1.1701	33.328	34.252	65	1.1308	25.789	26.505
83	1.1681	32.931	33.845	64	1.1287	25.392	26.098
82	1.1661	32.535	33.437	63	1.1267	24.996	25.690

Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.	Acid of 1.20 in 100.	Specific Gravity.	Chlorine.	Muriatic Gas.
62	1.1247	24.599	25.282	31	1.0617	12.300	12.641
61	1.1226	24.202	24.874	30*	1.0597	11.903	12.233
60*	1.1206	23.805	24.466	29	1.0577	11.506	11.825
59	1.1185	23.408	24.058	28	1.0557	11.109	11.418
58	1.1164	23.012	23.650	27	1.0537	10.712	11.010
57	1.1143	22.615	23.242	26	1.0517	10.316	10.602
56	1.1123	22.218	22.834	25	1.0497	9.919	10.194
55	1.1102	21.822	22.426	24	1.0477	9.522	9.786
54	1.1082	21.425	22.019	23	1.0457	9.126	9.379
53	1.1061	21.028	21.611	22	1.0437	8.729	8.971
52	1.1041	20.632	21.203	21	1.0417	8.332	8.563
51	1.1020	20.235	20.796	20*	1.0397	7.935	8.155
50*	1.1000	19.837	20.388	19	1.0377	7.538	7.747
49	1.0980	19.440	19.980	18	1.0357	7.141	7.340
48	1.0960	19.044	19.572	17	1.0337	6.745	6.932
47	1.0939	18.647	19.165	16	1.0318	6.348	6.524
46	1.0919	18.250	18.757	15	1.0298	5.951	6.116
45	1.0899	17.854	18.349	14	1.0279	5.554	5.709
44	1.0879	17.457	17.941	13	1.0259	5.158	5.301
43	1.0859	17.060	17.534	12	1.0239	4.762	4.893
42	1.0838	16.664	17.126	11	1.0220	4.365	4.486
41	1.0818	16.267	16.718	10*	1.0200	3.968	4.078
40*	1.0798	15.870	16.310	9	1.0180	3.571	3.670
39	1.0778	15.474	15.902	8	1.0160	3.174	3.262
38	1.0758	15.077	15.494	7	1.0140	2.778	2.854
37	1.0738	14.680	15.087	6	1.0120	2.381	2.447
36	1.0718	14.284	14.679	5	1.0100	1.984	2.039
35	1.0697	13.887	14.271	4	1.0080	1.588	1.631
34	1.0677	13.490	13.863	3	1.0060	1.191	1.224
33	1.0657	13.094	13.456	2	1.0040	0.795	0.816
32	1.0637	12.697	13.049	1	1.0020	0.397	0.408

At the density 1.199, Mr. Dalton's table* has 25.6 *per cent.* of real muriatic acid by weight, equivalent to only 32.9 chlorine, instead of 39.47, which I believe to be the exact value. If we term the correct quantity 100, then Mr. Dalton's number would be only 83; which is no less than 17 *per cent.* of defect from the truth. I have purposely omitted in this new table the column of dry, or, as it was also called, real muriatic acid; first, because there is no evidence at present of the existence of any such body;

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* *New System of Chemical Philosophy*, ii. p. 295.

and secondly, because, though it was a convenient column for finding by inspection the increase of weight which any salifiable base would acquire by saturation with the liquid acid, yet that convenience may be obtained by the following simple calculation. Since the prime equivalent of chlorine is to that of the supposed dry muriatic acid, in the ratio of 45 to 35, or 9 to 7; if we multiply the number opposite to the given specific gravity, in the chlorine column, by 9, and divide by 7, we shall have the relative quantity of the *fixable* muriatic acid.

Heretofore, several chemists have, without due consideration, assumed the half sum, or arithmetical mean of the specific gravities of two substances (water and acid, for example,) to be truly the *calculated* mean; and on comparing the number thus obtained with the *experimental* specific gravity of the compound, they have inferred the change of volume due to chemical combination. It is a singular fact, that the above arithmetical mean, which is always greater than the rightly computed mean, (see my paper on Mean Specific Gravity, in this *Journal*, vol. iv. p. 151,) happens to give in the case of liquid muriatic acid, an error in excess, very nearly equal to the actual increase of density. From this curious coincidence we derive a very simple rule for finding the approximate value of chlorine in the liquid acid at any density. Multiply the decimal part of the number representing the specific gravity by 200, the product will be the chlorine present in 100 parts. Thus, the specific gravity is 1.0437, what is the quantity *per cent.* of chlorine? $0.0437 \times 200 = 8.74$. Now the tabular number is 8.729. The sp. gravity being 1.059, what is the value of the chlorine in 100 parts? $0.059 \times 200 = 11.8$. The table has 11.9. Towards the head of the table this rule gives a slight error in excess; and towards the foot an equally slight error in defect, but the approximation is always good enough for ordinary practice, seldom amounting to one-half *per cent.* If to the number thus found for chorine we add $\frac{1}{36}$ part, the sum is the corresponding weight of muriatic acid gas.

ART. X. *On an easy and secure Method of Secret Correspondence.*

To the Editor of the QUARTERLY JOURNAL.

London, Dec. 11*th,* 1821.

SIR,

THE ingenious paper on secret writing, in your last Number, by Mr. Hincks, shews how little reliance can be placed on the methods suggested by Mr. Chenevix; and the dot-writing, as described in the elaborate article on Cipher, in Rees' *Cyclopædia,* is not only equally insecure, but would be liable to great perplexity if separate keys were furnished to several correspondents. Ministers and Generals, however, are often obliged to communicate in cipher with many agents at the same time, and perhaps with more than one person even in the same mission.

The mode to which I am about to advert was given to me many years ago by my father; but whether it has ever been published I do not know. Its pretensions are simplicity, impossibility of detection, and the capability of multiplying its keys to infinity.

Let the key for each of the correspondents be a line of poetry, or the name of some memorable person or place, that cannot be forgotten; and let them all be provided with a copy of the following table; which may be printed, as it matters not into whose hands it falls.

When the despatch is composed, write the key-sentence underneath the text, letter for letter, repeating it as often as it may be necessary. For example, suppose the key-sentence to be "Sir Humphry Davy," and the secret clause of the despatch to be "Protract the negociation as much as possible." These, when written as above directed, will stand thus:

Protract the negociation as much as possible
Sirhumph ryd avysirhumph ry davy si rhumphry

a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z
b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a
c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b
d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c
e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d
f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e
g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f
h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g
i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h
j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i
k	l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j
l	m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k
m	n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l
n	o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m
o	p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n
p	q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o
q	r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p
r	s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q
s	t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r
t	u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s
u	v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t
v	w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u
w	x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v
x	y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w
y	z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x
z	a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s	t	u	v	w	x	y

Now look in the left-hand vertical column for the first letter of the text *P*, and in the upper horizontal column for the corresponding letter of the key *S;* where these columns meet *h* will be found, which write down. Proceed in the same manner for the representation of the next letter *r*, (that is, at the junction of the columns *r* and *i*) which will be *z*; and so on through the whole of the paragraph. It will then appear as follows:

Hzfalmra kfh nzegkzhnudu rq puxf sa gvmexicc

The inverted process for deciphering needs no explanation.

I am, Sir, &c. &c.

B.

ART. XI. *A Translation of* REY'S *Essays on the Calcination of Metals*, &c.

[Communicated by JOHN GEORGE CHILDREN, Esq., F. R. S., *&c.*]

Continued from page 64.

ESSAY XV.

Air diminishes in weight in three ways. The balance is deceitful, the means of remedying that.

I RESUME the thread of my discourse, which I had somewhat interrupted to solve the objection advanced against me, and by so much the better clear up this matter; and I say, that in three preceding essays; *viz.*, the 10th, 11th, and 12th, I have shewn the three different ways by which air increasing in weight, may manifest that increase when balanced in a free and pure air. Now the law of contraries requires, that, by three ways, the reverse of the former, it may diminish in weight. These ways are the separation of any heavier foreign matter—its extension to more ample bounds, and the abstraction of its heavier parts. But since the knowledge of the former throws sufficient light upon the latter, I omit any fuller explanation; only begging the reader to observe, that this increase or diminution of weight, of which I have spoken in the said essays, always refers to a portion of air compared with another of equal bulk. For when we do not take the volume of a substance into account, if we examine its weight by reason, I say that nothing gains weight but by the addition of matter, nor loses it but by its subtraction—so inseparably are matter and weight united, as has been shewn above in the sixth essay. But if we investigate the subject by the balance, a case occurs in which, without any addition or subtraction of matter, a substance will appear more or less heavy; namely, by its contraction or expansion. Now this was the only mode of investigation known to the ancients when they contended that the elements, in their mutual conversion into one another, increase or diminish in weight in proportion as they increase or diminish in dimension, by the sole aid of nature, very expert in this operation—not that art is unable to increase or diminish the weight of things, dilating or contracting them. Hammer a piece of cold iron for a considerable time; you will unite its parts and diminish its bulk, and then it will appear heavier when put into the balance. Likewise, if you put a ball of feathers, closely tied up, into the scale, it will weigh more than when the feathers are left at their full size. From this I infer, what has been already cursorily touched on, that the balance is so fallacious, that it never indicates the true weight of substances, except when two portions of the same substance and figure, as two leaden bullets, are counterpoised against each other. But two ingots, one for instance of gold, and the other of iron, which appear by the balance to be equal, are nevertheless not so—for the iron is as much heavier than the gold, according to reason, as the air which it displaces is heavier than that displaced by the gold, which difference I could shew exactly in every thing we weigh, and could reduce the whole to its just weight, if I had made the trial I have suggested above in the 7th essay.

ESSAY XVI.

Formal answer to the question, Why Tin and Lead increase in weight when they are calcined[*]*?*

I have now made the preparation; laid as it were the foundations of my answer to the Sieur Burn's demand; namely, that having put *two pounds six ounces* of fine English tin into an iron vessel, and heated it strongly on an open fire for six hours, stirring it continually, without having added any thing, he obtained *two pounds thirteen ounces* of a white calx; which at first occasioned him great surprise, and the desire to ascertain whence these seven ounces of increase were derived.—Now, to augment the difficulty, I say, that we must not only inquire whence these seven ounces are derived, but, moreover, whence that which has replaced the loss of weight, necessarily arising from the enlargement of volume of the tin, by its conversion into calx, and from the vapours and exhalations that have escaped. To this question, then, resting on the foundations I have laid, I answer, and proudly maintain, "That this increase of weight comes from the air, thickened and made heavy, and in some measure rendered adhesive in the vessel by the violent and long-continued heat of the furnace—which air mixes with the calx (its union being assisted by the continual stirring), and attaches itself to its smallest particles—no otherwise than as water, when sand is thrown into it, makes it heavier by moistening it, and adhering to its smallest grains." I imagine there are many persons who would have been *startled (effarouchées)* at the mere statement of this answer, had I given it in the outset, that will now receive it willingly, being, as I may say, *tamed (apprivoisées)* and rendered tractable, by the evident truth of the preceding essays. For doubtless they whose minds were preoccupied with the opinion that air is absolutely light, would have rushed to the encounter, exclaiming, Why do we not extract heat from cold, white from black, light from darkness, if from air, a thing absolutely light, we can extract so much weight? And they, who might have given credit to the weight of air, would have been unable to persuade themselves, that it could ever increase the weight of a substance balanced in itself.

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[*] *Note by M. Gobet.*

John Baptist Porta, a Neapolitan gentleman, author of several works on Natural History and Agriculture, has recorded the same fact.

"Plumbum ponderosius reddere, docet Galenus. Nam comprobans plumbum particeps esse aëreæ substantiæ, hoc affert experimentum. Omnium quæ novimus, unicum plumbum, tum mole ipsâ, tum pondere augetur, si condatur in ædibus subterraneis, aërem habentibus turbidum, ita ut quæcumque illic ponantur, celeriter situm colligant. Tum etiam plumbea statuarum vincula, quibus earum pedes annectuntur, sæpe numero crevisse visum est, et quædam adeo intumuisse, ut ex lapidibus dependerent crystalli, modo verrucæ." (Lib. v. cap. 11.)—***Magiæ Naturalis***, libri viginti.—Hanoviæ, 1619.

Bacon says the same,—(*Sylva Sylvarum.*)

On this account I have been obliged to shew, that air is possessed of weight; that it is proved by other investigation than that of the balance; and that even by that instrument, a portion previously altered and thickened, may make its weight manifest. This I have done as briefly as possible, without having advanced any thing not bearing fully on the subject; to clear up which, in every point, it only remains to give a succinct account and refutation of the opinions which others have followed, or follow; and to solve the objections that might be made to my answer.

ESSAY XVII.

It is not the disappearance of the celestial heat which animates the Lead, or the death of the latter that increases its weight in calcination.

Of all, as far as I know, that have written on this question, Cardan is the first, who, in his fifth book, *De Subtilitate* *, says, that lead, by conversion into ceruse, or by calcination, gains one-thirteenth part in weight, and gives this reason for it—The lead dies, for the celestial heart, which was its soul, vanishes; whose presence gives it life, and renders it light; as its absence occasions its death, and makes it heavy. This he confirms by the example of animals, which become heavier after death, from the extinction of the celestial heat, the soul, (as he thinks), both of animals and all other mixed and compound bodies. This opinion is defective, to say no worse of it, in many respects. First, in attributing life to lead. Secondly, in supposing that the presence of the celestial heat makes it light, and its absence heavy. Thirdly, because it assigns the same reason for the increased weight of lead by calcination, and of animals by death. There is nothing of the kind. For as to life, how can lead possess it, since it is a homogeneous body, without difference of parts, without organs, and without any vital effect or action? If it move downwards, so does ceruse, which is only its corpse; if it *be cooling* (*rafraischit*), so is ceruse. Then how could it preserve this life, under a million of forms, that it may be made to assume and to cast off, yet always continuing to be lead? How, in the furnace (which would be a much greater wonder), where it may be kept in fusion a day, a month, or a whole year? It must have a very tenacious soul to undergo so much without being dislodged! Moreover, all the world is agreed, that from death to life there is no return. Yet the chemists assure us, if we moisten the calx of lead, and mix it with water in which samphire (salicot) has been dissolved, then, having dried it, put it in a crucible with a small vent, and heat strongly and quickly, that we shall reduce it to its original state. With regard to the celestial heat making bodies light, Scaliger* very properly objects that the heavens, which abound in this heat, as being the source of it, *must be light (feut leger)*, and consequently *univocal* (*univoque*) with the other bodies, which is absurd. Neither can the loss of this heat render them heavy, for I have already proved that nothing increases in weight but by the addition of matter, or by diminution of volume; but here there is nothing of the kind; so that the disappearance of the heat cannot add any thing, and as to its bulk, it is visibly enlarged; the compact and solid substance of the lead being reduced (*amenuisée*) to so many small parcels, that their number is almost infinite. Plants too ought to become heavy by death, the celestial heat being expelled: but the contrary is evident to all. As to the increased weight of animals by death, the true cause, far remote from that which increases the weight of lead when calcined, is this: in the living animal its natural heat subtilizes, dilates, and augments the dimensions of the humours, the flesh, and every thing in it capable of dilatation—but losing this heat by death, the whole on this becoming cold, contracts and diminishes, whence the increase of weight, as I have often said already. What is there like this in lead? Thus the opinion of Cardan appears so frivolous, that I am grieved that a great man, and one who is justly esteemed by all the world, should have lately declared to me that he inclines towards it.

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* *Note by M. Gobet.*

"Nam plumbum cùm in cerussam vertitur, ac uritur, tertiâ decimâ parte sui ponderis augetur. Hoc fit, quia calor ille cœlestis evanescit: nam certum est, adjici nihil, et tamen crescit: cùm igitur par ratio etiam in animalibus videatur, quæ graviora morte fiunt, quoniam exhalante animâ, secum calor etiam, ac quicquid ab illo est elaboratum evanescit: manifestum est corpora metallica, et lapides ipsos etiam vivere." (Lib. v. *de mistione et mistis.*)—Cardani, *de Subtilitate*, lib. xxi. Parisiis, 1551.

This author also observed, that a tile gains weight in burning.

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* "In lapide, inquis, vita.—Quia ubi calor, ibi anima. Ubi anima, ibi vita. Igitur in lapide vita. Ostende mihi tu in lapide calorem, et ego ostendam tibi calorem sine animâ. Fortasse negabis in igne calorem; animâ carere non negabis. Nam quod ais, omnem actionem ab animâ provenire, si verum est, dabis animam etiam cerusæ. Descendit enim. Ergo per terræ formam, quæ est ibi terræ anima, quâ movetur. Sic enim et sapis, et scribis. At ais, mortuam cerusam, quia facta est è plumbo, à quo calor ille cœlestis exhalavit. Calorem hunc, inquam, ostende nobis. Nam calor, non nisi tactu cognosci potest. Equidem lapidis contactu nunquam pilo factus sum calidior. Effectionem, deces, nempe descensionem. Nego tibi fieri à calore. Si enim à calore fieret descensio, à calore non fieret, ascensio. Exemplum verò de cerusa ridiculum. Cerusâ, inquis, plumbo gravior: quia calor abiit cœlestis ille. Ergo cœlum habet levitatem, et erit quintum corpus cum aliis univocum. Namque viventia, O! subtilissime Philosophorum, non propter animam levia sunt, sed propter calorem elementarem. Alioqui omnia tua entia essent levia. Quippe vivunt omnia. Vivunt autem propter formam. Forma illa anima, anima illa calor. Calor ille autor levitatis. Levia igitur omnia, quia materiæ forma dominatur: et cœlestia potiora, atque efficaciora. Tactu autem percipimus calorem elementarem. Quem in plumbo qui sentit tactu, sentiat etiam in igne frigus. At plumbum absumptis partibus aëreis gravius fit. Quâ de causâ later quoque coctus crudo gravior. At contra, arundo combusta levior fit. Verè tamen illa, atque sine controversià viveb At ejus abiit anima; ita de aliis arboribus dicendum. Igitur anima neque gravis, ne-

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que levis; neque cœlum ipsum. Id quod è secundo de animâ, primo et secundo de cœlo colligi potest. Ideo cerusa friabilis, amisso pingui aëreo. Tum quæro de te plumbum quomodo vivum sit. Namque est excoctum igni, sicuti cerusa aceto excocta est. Ignis autem destruit: ut passim, quiritaris, apud te vixerit in fodinâ, in apothecis, in fornacibus interemptum nequit vivere. Alioquin ab igne nostro cœlestis ille tuus esset generatus." (Exercit. ci. cap. 18.) And further on, he says, "Plumbum quoque aiunt augescere. Calcem in fornace tectâ vidimus adeò turgescere, ut tigna atque tegulas sustolleret."

Jul. Cæs. Scaligeri, *de Subtilitate*, &c. Francofurti, 1592.

[To be continued.]

ART. XII. *Proceedings of the Royal Society.*

THE meetings of the Royal Society were resumed on Thursday the 8th of November, after the long vacation. At this and the succeeding meeting the *Croonian Lecture* was read by Sir Everard Home: it contained an account of the means by which the eye adjusts itself to the distinct vision of near and remote objects.

Thursday, *Nov.* 22. The *Bakerian Lecture* by Captain Edward Sabine, was read; it contained an account of experiments to determine the dip of the magnetic needle in London in August 1821, with remarks on the instruments usually employed in such determinations.

Friday, *Nov.* 30, being St. Andrew's day, the Society held their anniversary meeting. At this meeting the President announced the allotment of two of Sir Godfrey Copley's prize medals to J. F. W. Herschel, Esq. and Captain Edward Sabine, in a very appropriate and impressive discourse, of which the following is an outline.

The progress of discovery, said Sir Humphry, even when belonging to past times or distant countries, is always an agreeable subject of contemplation to philosophical men, but the pleasure derived from it is much higher when it arises from the exertion of the talents of our own countrymen, when it has originated in our own body, and when there is the power not only of acknowledging and rejoicing at it, but likewise of distinguishing the persons to whom it is owing, by a permanent mark of respect; he, therefore, had much satisfaction in announcing the decision of the Council of the Society, upon the present occasion. The President then took a review of the labours of Mr. Herschel, and more particularly of those which were considered as entitling him to the present mark of distinction. He said, there was no branch of science more calculated to awaken our admiration than that which Mr. Herschel had so successfully cultivated: the sublime or transcendential geometry not only demonstrated the powers and resources of human intellect, but also the wisdom and beauty of the laws which govern the universe. It is, perhaps, said the President, the highest triumph of human intelligence, that proceeding from the consideration of mere unities, or points, lines, and surfaces, it should by gradual generalizations, substitutions, and abstractions, be able to arrive not only at the knowledge of all possible conditions of number and quantity, but likewise of time and motion; and by employing its own pure intellectual creations, in many cases anticipate the results of observation and experiment, and determine the movements not only of the bodies which form permanent parts of our system, but likewise of those which seem only occasionally to visit it, and which belong as it were to the immensity of space.

Sir Humphry then paid an eloquent tribute of applause to the zeal and success with which Mr. Herschel had pursued these inquiries, and proceeded to enumerate his mathematical communications to the Royal Society, printed in their Transactions. He should not, he said, attempt an analysis of these papers, for they required profound study; they were especially distinguished by the simplicity of the processes, by perspicuity of arrangement, and by the absence of all metaphysical abstractions, and they proved in the author an intimate acquaintance with the works of the great masters of analysis; he had not, however, limited himself to formulæ, but had a higher claim upon the approbation of the Society in their application; for, though as a mere exercise, the higher mathematics strengthen the reasoning faculties and afford intellectual pleasure, yet it is in enabling us to solve the physical phænomena of the universe that they have their grandest end and use; in these respects, said Sir Humphry, they are really power, and they may be compared to that power which we witness in the vapour of water, which passing into the free atmosphere, exhibits only a striking spectacle, but which applied in the steam-engine, becomes the moving principle of the most useful and extensive machinery, and the source of the most important arts of life.

Sir Humphry then adverted more particularly to Mr. Herschel's investigations connected with the polarization of light, and to the importance in practical optics of his paper on the aberration of compound lenses and object-glasses; and having stated to the Society the leading features of these communications, said that he felt convinced they could not but approve of the decision of their Council upon the grounds he had already mentioned, and should therefore not attempt further to expatiate upon the diligence and merits of their author.

In addressing Mr. Herschel personally, Sir Humphry alluded to several subjects of inquiry which he recommended to Mr. Herschel's attention; he trusted that he would continue to devote himself to philosophical pursuits, and that he would receive the medal not merely as a mark of respect for acquirements already made, but as a pledge of future exertions in the cause of science, and of the Royal Society. "Believe me," he added, "you can communicate your labours to no public body by whom they will be better received, or through whose records they will be more honourably transmitted to the scientific world. And these pursuits, you will find not only glorious, but dignified, useful, and gratifying in every period of life; this, indeed, you must know best in the example of your illustrious father, who, full of years and of honours, must view your exertions with infinite pleasure, and who, in the hopes that his own unperishable name will be permanently connected in the annals of science with yours, must look forward to a double immortality."

The President then spoke of the researches of Captain Edward Sabine; he eulogized his industry and perseverance in conducting his inquiries in the Arctic Expedition, and his fortitude and patience in enduring the hardships and privations to which he was then exposed: his experiments he said, were principally conducted on the ice of the polar sea, where the vessel was for several months frozen up; during a considerable portion of the time, he was in darkness, or only guided by a very doubtful twilight, and the temperature of this inclement spot, probably as cold as any belonging to the northern hemisphere, was such, that the artificial horizon of mercury became frozen during an observation; yet Captain Sabine's inquiries seem to have been conducted with as much care and precision as if he had been possessed of the conveniencies and luxuries of a Royal Observatory, and the advantages and repose of the happiest climate and situation.

Sir Humphry then entered into some historical details respecting the researches connected with the length of the seconds' pendulum in different latitudes, and its application to determine the figure of the earth. The true investigation of the properties of the pendulum as an universal standard of measure was owing he said to the zeal and enlightened views of Mr. Davies Gilbert, who had directed all the scientific talents and resources of the country to the object, by making it a question of national importance in parliament; the fortunate result had been the invariable pendulum contrived with such a happy spirit of invention, and examined with such unceasing activity and minute accuracy, by Captain Kater.

"Captain Sabine did not accompany the Arctic expedition now absent, because," said the President, "he conceived that he had effected all that he was capable of performing with the pendulum in northern latitudes, which was the great object of his researches in the two former voyages; but his scientific ardour had induced him to endeavour to complete his investigations, even to the Line; and having braved the long night and perpetual winter of the Polar Regions, he was now gone with the same laudable object to expose himself to the burning sun and constant summer of the Equator."

In Captain Sabine's absence, the President delivered the medal to his brother. "In informing him," said Sir Humphry, "of what has taken place this day, you will, I trust, state to him our deep sense of his merits; his knowledge of this expression of our opinion may, perhaps, animate him during the difficult enterprise he has undertaken, for he has already shown how highly he values the praise of the Royal Society, which, with the good opinion of his countrymen, has been hitherto the only reward of his labours. Assure him how strongly we feel his disinterestedness and genuine love of science, and that our ardent wishes are expressed for his safe return, and for the successful accomplishment of the objects of his voyage, which will ensure to him additional claims upon the gratitude of all lovers of science."

The Society then proceeded to the election of a Council and Officers for the year ensuing. On examining the lists, the following was found to be the state of the elections:

1. *Members of the Old Council to continue for the ensuing Year.*

Sir H. Davy, Bart.
William Thomas Brande, Esq.
Samuel Goodenough, Lord Bishop of Carlisle.
Taylor Combe, Esq.
Davies Gilbert, Esq.
Charles Hatchett, Esq.
J. F. W. Herschel, Esq.
Sir Everard Home, Bart.
John Pond, Esq.
William Hyde Wollaston, M.D.
Thomas Young, M.D.

2. *Members of the Society chosen into the Council.*

George, Earl of Aberdeen.
Matthew Baillie, M.D.
John Barrow, Esq.
B. C. Brodie, Esq.
William Hamilton, Esq.
James Ivory, Esq.
Henry Marquis of Lansdowne.
Alexander Marcet, M.D.
Thomas Murdoch, Esq.
Sir Robert Seppings, Knt.

3. *Officers for the Year ensuing.*

President, Sir Humphry Davy, Bart.
Treasurer, Davies Gilbert, Esq.
Secretaries, { William Thomas Brande, Esq.
Taylor Combe, Esq.

Thursday, Dec. 6.—A paper was communicated by the Society for the Improvement of Animal Chemistry, entitled, "On some Alvine Concretions found in the Colon of a young Man, in Lancashire, after death." By John George Children, Esq. F.R.S.

Thursday, Dec. 13.—A paper was read, " On the Concentric adjustment of a triple Object Glass." By W. H. Wollaston, M.D. and V.P.R.S.

On the same evening a communication was also read, entitled, " On a new species of Rhinoceros, found in the interior of Africa; the skull of which bears a close resemblance to that found in a fossil state in Siberia, and other Countries: by Sir Everard Home, Bt., V.P.R.S."

Thursday, Dec. 20.—A paper was read on the Electrical Phenomena exhibited in vacuo, by Sir H. Davy, Bart., P.R.S.

The Society then adjourned over the Christmas Vacation, to meet again on Thursday, the 10th of January, 1822.

ART. XIII. *Account of the Progress of Foreign Science.*

THE daily increasing intercourse between this country and foreign nations, has enabled us to extend and systematize this valuable department of a scientific journal. In future, the account of foreign science will be presented in the following order:

I. CHEMICAL SCIENCE. 1. Principles of combination. 2. Repulsive power or caloric. 3. Simple bodies. 4. Inorganic compounds; acids, bases, salts, *&c.* 5. Organic compounds, vegetable and animal. 6. Analysis and chemical apparatus.

II. APPLICATIONS of CHEMISTRY, to Medicine and the Arts; Agriculture, Bleaching, Dyeing, *&c.*

III. PHYSICS; Optics, Electricity, Magnetism, Acoustics, *&c.*

IV. MECHANICS. 1. Statics, Dynamics. 2. Hydrostatics and Hydrodynamics. 3. Ærostatics and Pneumatics.

V. NATURAL HISTORY; Zoology, Botany, Mineralogy, Geology, Meteorology.

VI. ECONOMICS; or miscellaneons applications of Science to the ordinary uses of life.

The deficiencies in any of the above departments which may exist in one Number, we shall endeavour to supply in another.

I. *Chemical Science.*

1. **Principles of Combination.** It gives us pleasure to observe, that the accumulation of new facts in favour of the chloridic theory first promulgated by Sir Humphry Davy, has at length forced something like an assent to its truth, from the ablest and most obstinate partisan of the French oxymuriatic hypothesis. Professor Berzelius, after waging a war of obscure and ineffectual words, like Priestley with Phlogiston, against the English system, says, in his late paper on the Sulpho-cyanides: " What I have just stated, both on the cyanides in my preceding memoir, and on the sulpho-cyanides in the present, proves that the phenomena relative to these substances, *can admit of explanation only*, on a theory quite analogous to that which in these later times has been adopted for the muriates, in considering oxymuriatic gas, as a simple body-chlorine. This theory becomes more general and at the same time more interesting, by extending it to all the acids combined with water (hydrates), as well as to all the salts, in the manner proposed by M. Dulong, by considering the acids and the salts, as compounds of hydrogen and of metals combined with the radical of the acid, and the oxygen both of the acid and of the base, together forming one body." " The decompositions and the recompositions of water, which I have conjointly with other chemists, regarded as a great objection against the new theory of the nature of muriatic acid, take place with the sulpho-cyanides in a manner which leaves no doubt of their reality; and, we may add that the cyanides, and especially the sulpho-cyanides, have a perfect analogy with the salts formed by oxidized bases, and oxygenated acids; that is to say, formed by a combustible radical and oxygen*." This *Palinodia* would have been better without the errors tacked to it. The above view of the constitution of acids and salts was given long ago by Sir H. Davy, as may be seen in his paper, " On the analogies between the undecompounded substances, and on the constitution of acids," published in the first volume of this Journal. Thus; " it is a simple statement of facts to say, that liquid nitric acid is a compound of two proportions of hydrogen, one of azote, and six of oxygen; and, as I shewed long ago the only difference between nitre and hyperoxymuriate of potash is, that one contains a proportion of azote, and the other a proportion of chlorine," p. 287. And, with regard to these decompositions and recompositions, they are mere fictions conjured up by himself and the late Dr. Murray. Chlorine dislodges oxygen from potash, soda, lime, *&c.*,

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* Annales de Chim. et de Physique, Tome XVI, pp. 35, 36, et 37.

as we know by indubitable experiments; and, what reason is there to fancy or to assert, that on putting any of these chlorides into water, the oxygen of the latter displaces the chlorine, which goes to the liberated hydrogen to form muriatic acid? In fact, the water quietly combines with a chloride as it does with an oxide; so that the ghost is entirely of their own raising, and they may lay it at their leisure. If heat be applied, indeed, then a decomposition may be induced under new circumstances, as happens with some peculiar chlorides.

The jealousy of English predominance in European affairs, which was somewhat natural, and perhaps excusable in a military and political point of view during the late war, is quite absurd and ludicrous, as now displayed by many eminent continental chemists with regard to the theory of chemical equivalents, or the English atomic theory. Berzelius and many other philosophers, whom we could name, still continue to refer all their analyses of oxides and salts to a supposed principle, that the oxygen in the oxides is a sub-multiple, by a whole number, of the oxygen in the acids. Now, this shews a very limited and imperfect comprehension of the general laws of equivalent combination, as taught by Mr. Dalton, and as represented on Dr. Wollaston's scale. In fact, that supposed *principle* of Berzelius, is merely a partial and accidental corollary of the great system of chemical proportions. Thus, as carbonic acid contains two atoms or proportions of oxygen, its combinations with protoxides will give the ratio of oxygen in it, to that in them, of two to one; but with deutoxides, the ratio of equal quantities. Again, as sulphuric acid contains 3 atoms or proportions of oxygen; its combinations with protoxides will furnish the ratio of oxygen in it, to that in these bases, of three to one; but with deutoxides, the ratio of 3 to 2, or 1½ to 1. And, as nitric acid contains 5 atoms or proportions of oxygen, its combinations with protoxides will give the ratio of oxygen in it, to that in these bases, of 5 to 1; but with deutoxides, the ratio of 5 to 2; or 2½ to 1; or that of 1 to 0.4. Hence this partial conception and imperfect representation of the theory of equivalents, leads to very confused and perplexing results, which we altogether avoid, by tracing up the ratio for every simple or compound body, directly to its place in the general scale of proportions.

We shall give an example or two from a very recent and otherwise very valuable paper of Berzelius, "on the composition of several inorganic combinations, which serve as the base of calculations, relative to the theory of chemical proportions *." "Bucholz found by a series of very exact experiments, that the sulphuret of molybdenum contains 60 parts of metal to 40 of sulphur; and, that these 60 parts of metal absorb 30 parts of oxygen to become molybdic acid. It follows that for molybdenum, there is between the sulphur and the oxygen of the acid a ratio analogous to that which we have found, for the sulphuret and the acid of arsenic; that is to say, the sulphuret of molybdenum is proportional to a degree of oxidation inferior to that of the acid." Now, this proposition is true no doubt, but very much mystified in the enunciation. The sulphuret of molybdenum is evidently a compound of one atom metal + 2 atoms sulphur; or is a bisulphuret: and the acid is like all the well-marked metal acids*, a compound of one atom metal + 3 atoms oxygen. Hence the ratio of 4, (=2 × 2) to 3 as found by Bucholz.

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* Annales de Chim. et de Physique, Tome XVII, p. 6, et seq.

Ten grammes of nitrate of lead, equivalent to 6.731 oxide of lead, yielded of molybdate 11.068 grammes, according to Berzelius. He then proceeds: " By a very simple calculation we find that the molybdate of lead is composed of—

Molybdic acid	39.185	100.0
Oxide of lead	60.815	155.2

Now, 155.2 parts of oxide contain 11.129 parts of oxygen, which are a submultiple of 50 (the quantity of oxygen absorbed by 100 of molybdenum, in the experiments of Bucholz,) by 3; for 11.129×3=33.387. It follows thence that, in the neutral molybdates, the acid contains three times as much oxygen as the base. It is then composed of—

Molybdenum	66.613	100.00
Oxygen	33.387	50.12

Now, this marvellous prolixity and obscurity of induction, might have been all saved, by referring molybdenum directly to its place in that general scale of equivalents of which oxygen is the *radix*, or *modulus*. He would also have avoided the unwarranted position with which the quotation concludes, of the acid containing three times as much oxygen as the base; for molybdic acid forms an insoluble precipitate with some of the deutoxide salts of copper, which compound consists, in all probability, of an atom of acid to an atom of base; and therefore the former will contain a quantity of oxygen, to the latter in the ratio of 3 to 2, and not, as he says, of 3 to 1. Again, when treating in the same paper of the composition and the capacity of saturation of chromic acid, Berzelius says, " It thence follows that the chromate of lead ought to be composed of—

Chromic acid	31.853	100.000
Oxide of lead	68.147	213.924

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* Thus, as to the acids of Chromium, Manganese, Arsenic, Tungsten, consist each of one atom metal + 3 atoms oxygen. Selenic acid has two atoms, but its base can hardly be called a metal. Antimonic acid has also probably two; but it resembles an oxide more than an acid.

Now, the 213.924 parts of oxide of lead contain 15.34 parts of oxygen. This number, then, expresses the capacity of saturation of the chromic acid." That is to say, that weight of every base which 100 parts of chromic acid neutralize, must contain 15.34 parts of oxygen. Again, from the decomposition of Siberian chromate of lead, he thus argues: "The loss in this experiment can be nothing but oxygen; so that 24.25 parts of green oxide of chromium will afford, with 7.37 parts of oxygen, 31.62 parts of chromic acid. Now, the oxygen contained in the 68.38 parts of oxide of lead is 4.9, which, multiplied by 1½, gives a product of 7.35; that is to say, chromic acid, when it is reduced to the state of green oxide, loses a quantity of oxygen, equivalent to 1½ times its capacity of saturation." "The analysis" (of chromate of barytes) "has then given—

Barytes .	59.68
Oxide of chromium .	30.43
Loss .	9.69

These 59.88 parts of barytes contain 6.26 parts of oxygen, and $6.26 \times 1\frac{1}{2} = 9.39$. The result of this analysis accords, therefore, with that of the preceding experiment (on the Siberian chromate.) The small difference (=0.3,) can be ascribed only to an error of observation, inseparable from this mode of operating."

Now, we must confess that, though the experiments were judicious and accurate, and though the reasoning be ultimately not untrue, yet it is presented in so involved a manner as to create very superfluous confusion to the reader. Instead of hanging his results upon a little peg of a corollary to the system of equivalents, why does he not assign them their due position in the general fabric, so that we could at once see their relation to every other chemical body, on the comprehensive plan of Richter? Direct experiments shew that chromic acid has the equivalent weight of 6.5 on the oxygen scale (or 6.54 by the above synthesis of the chromate of lead;) and that it contains 3 atoms of oxygen to one of metal, while its green oxide seems to consist, by the preceding experiments, of 3 atoms of oxygen to 2 of metal. On this point we shall say a word or two in its place; our sole object here being the general principles of combination.

M. A. le Roger et J. A. Dumas, Pharmaciens, have subjected a number of bodies to a nice hydrostatic process, in order to determine their specific gravity very exactly, and thence deduce a general atomic law. They employed both alcohol and oil of turpentine for the liquids of immersion. They justly observe that M. Hassenfratz, by employing mercury as the hydrostatic liquid in the examination of salts, fell into gross errors, such as that caustic lime and calcined alum were lighter than water, errors which are ingrafted, without comment, into the largest system of English chemistry. The above experimentalists imagine that, on considering the quotients obtained by dividing the weights of the atoms by the respective specific gravities of the bodies, they find the volumes of the atoms to form a series, which pervades the greater number of simple substances. Although we conceive this series to be a matter of illusion, yet as the hydrostatic experiments seem to have been executed with care, we shall insert their first table of results.

Name of the Substance.	Weight of the Atom.	Density reduced to the Vacuum and at 39.2° Fah.	Volume of the Atom.	Ratio.	Volume of the Atom by Calculation.
Ice *	112.4354	0.950	117	1	116
Silica	596.42	2.650	225	2	232
Boracic acid	269.65	1.830	220	2	232
Arsenious acid	1240.77	3.698	335	3	348
Protoxide of copper	891.39	5.749	155	1⅓	155
Oxide of bismuth	1973.8	8.449	233	2	232
Oxide of lead	2789.0	8.010	347	3	348
Peroxide of mercury . . .	2731.6	11.29	240	2	232
Caustic lime	712.06	3.08	231	2	232
Carbonate of lime	1262.72	2.717	464	4	464
Solid carbonic acid in the carbonate of lime	. . . . .	. . .	116	1	116
Anhydrous sulph. lime . . .	1714.38	2.960	579	5	580
Solid sulph. acid in sulphate of lime	. . . . .	. . .	174	1½	174
Crystallized sulph. of lime .	2164.12	2.322	932	8	928
Alumina	642.32	4.200	152	1⅓	154
Nepheline, silicate of alumine	1238.75	3.270	378	3⅓	386
Water in cryst. sulph. lime .	. . . . .	. . .	88	¾	88
Sulphur	. . . . .	2.086	. .	.	. .

In the preceding table, the common divisor employed by these gentlemen is 116= their volume of the atom of ice. Their atomic weights are mostly taken from Berzelius. They seem perfectly unacquainted with Dr. Prout's happy generalization; which renders it more than probable that the weight of all the atoms are multiples of the atomic weight of hydrogen, by a whole number. Indeed, we have little reason to be surprised at this ignorance in these gentlemen, when M. Berzelius

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* Our experiments on the sp. gr. of ice give the number 0.9133. It was taken in oil of turpentine, at 32° Fahr.—*Trans.*

and M. Dulong, two of the most distinguished chemists on the Continent, in the sequel of their late elaborate memoir on the specific gravity of some elastic fluids [*], speak of Dr. Prout's speculations in the following words :—" Before concluding we shall observe, that the new determinations, which we now present, differ little from those which are to be found in an anonymous memoir, printed in the *Annals of Philosophy*, for November 1815, and February 1816. But the English author has made no experiments, and the hypotheses, which he employed to correct his adopted numbers, being absolutely gratuitous or false, his results could not inspire any confidence." This we consider as a harsh and unjustifiable criticism. Can any thing be a better proof, that the hypotheses were *not* gratuitous and *not* false, than that *after the* progressive investigations of five years, two French chemists, by applying the most refined and rigid methods of experiment, should arrive at precisely the same results, which the English author had theoretically deduced ?

II. CALORIC.—The principal paper on this subject, which has recently appeared on the Continent, is that of M. Navier, Professor of Practical Mechanics in the *Ecole des Ponts et Chaussées* of Paris; and it is entitled, " Note on the Mechanical Action of Combustibles [†]." Several writers have endeavoured to establish a comparison between the mechanical actions, capable of being produced by the same weight of a combustible, employed to vaporize water, and to heat atmospherical air. It has been advanced, that the latter process, making allowance for the variable causes of loss which occur in machines, might be more advantageous than the former. Several artists have endeavoured to construct machines on this principle [‡].

In the calculations presented on this question, there exists a cause of error, which has been remarked by M. M. Clement and Desormes, in a memoir presented to the Academy of Sciences, in 1820, but not yet printed. The authors of those calculations had not taken into account, the quantity of heat which the air absorbs, when it dilates, without changing its temperature. Mr. Navier takes for his unity of heat the quantity necessary to raise by 1° C., the temperature of a kilogramme (about $2\frac{1}{4}$ pounds avoirdupois) of water in the liquid state ; this unity he styles a *degree of heat*. The number of *degrees of heat* necessary to raise by 1°, the temperature of 1 kilogramme of air, of which the volume, which was w at the temperature 0°C., under the atmospheric pressure $0^{m}76$,= (29.92 English inches), has become O, he represents by the following formula :—

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[*] *Annales de Chimie et de Physique.* tom. xv. p. 386.

[†] *Ibid*, tom. xvii. p. 357.

[‡] By far the most ingenious of these is the heated air-engine, for which a patent was, a few years back, granted to the Rev. Mr. Sterling, of Kilmarnock. Of this we shall give a short account in a future Number.

$$0.26\sqrt{0.76\frac{o}{w}+0.24}$$

By means of these results, he seeks to establish a relation between the quantity of action (power) which it is possible to obtain, in heating and dilating the air, and the quantity of heat which is consumed. When the volume of the atmospheric air is thus made to vary, the quantities of heat, which it absorbs or disengages, are proportional to the variation of the specific heat. So that, calling C the primitive specific heat, c the specific heat after the change of volume, we have for the quantity absorbed; a, (c ——— C), a being a constant co-efficient, whose value in round numbers may be stated at 1200°. After a train of algebraic formulæ which we cannot here insert, he concludes, that in expending a degree of heat to warm the air, we cannot obtain a quantity of action which surpasses the elevation of a weight of 33 kilogrammes (72.8 libs.) to the height of one metre; considering always 500° C. (= 932° F.) as the highest temperature to which it can be carried. In treating of the comparative powers of heat, when employed in the vapour of water, he endeavours to shew, that there is an advantage in producing the vapour at the highest possible temperature. Supposing the vapour produced under the pressure of 5 atmospheres; that is, let H (the elastic force of the vapour) = $3^{m}.8$ (149.6 E inches), and V (corresponding temperature = 165° C. or 329 F *. The value in this case most suitable to V′ (temperature of the condensed water), as may be found by calculation, would be less than 10° C. (50° F.) which has been adopted for the exterior temperature v. Suppose, however, that V′ = 10° C. and of consequence H′ (the elastic force of vapour corresponding to V′ or 10° C.) = $0^{m}.0095$. our formula will then give for the *maximum* of the quantity of action which it is possible to obtain,

123300 Π kilogrammes × metres; and for the *minimum* of the corresponding expenditure of heat; 685 *degrees.*

The ratio of these two numbers being 180, we see that by expending a degree of heat to produce aqueous vapour, the limit of the quantity of action which it is possible to obtain, is the elevation to one metre in height, of a weight of 180 kilogrammes

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* This temperature seems to be calculated from some erroneous formula of the force of steam. The temperature corresponding to five atmospheres, by Dr. Ure's experiments, is only 305 F., or 151.6C.

(368.56 lbs. avoirdupois), supposing the steam generated under the pressure of five atmospheres *.

From these results the theoretic limits of the quantities of action which it is possible to obtain in heating air, or water, are nearly to each other in the proportion of 33 to 180; or of 1 to $5\frac{1}{2}$. The difference is so considerable, that, notwithstanding the uncertainty which may exist in some of the preceding numerical values, the preference due to steam appears to be, in no respect, doubtful.

But steam engines present always a very considerable abatement under the theoretic *maximum* of effect, now computed. It appears that the best steam engines, possessing the average power of ten horses, consume $2\frac{1}{2}$ kilogrammes ($5\frac{1}{2}$ lbs. avoirdupois) of charcoal (coal?) to obtain a quantity of action, equal to 28800 k. × m. Admitting that a kilogramme of charcoal consumed in the calorimeter gives 7000° of heat, we see that we obtain, for one degree of heat, only a quantity of action $= \frac{28800}{2.5 \times 7000} = 16.5$ kilog. × met.; which is very far from the limit 180 k × m. found above. It would be important to investigate and to appreciate exactly the causes of this considerable loss. We hope to see many of these important problems, referred to by M. Navier, treated in a clear and conclusive manner, in M. Clement's new Treatise, Theoretical and Practical, on Heat, and its applications to the Arts.

We are indebted to M. Navier, also, for an ingenious memoir, in the same number of the *Annalés,* on the variation of temperatures, which accompanies the changes in the volume of gases. His reasoning is founded on the experiments of M. M. Clement and Desormes, Delaroche, and Berard. From his formulæ it appears, that on reducing the volume of air to $\frac{1}{5}$, so that the pressure becomes 149.6 E inches, we can obtain an elevation of temperature of about 237° C. (426.6 F.)

It further appears, from his mode of research, that the rise of temperature obtained by the condensation of atmospheric air, is susceptible of a limit, somewhat confined. The formula gives 360° C. as the *maximum* effect. As to the cold producible by the dilatation of air, the formula does not assign it any limit.

M. Flaugergues, the astronomer, has made, at the Observatory of Viviers, a series of nice observations on the heat produced by the sun's rays, during the eclipse of this luminary,

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* The formulæ for the *maximum* is,

$$\pi.\ 0.76.\ \frac{\Pi}{0.59}\cdot\ \frac{1 + 0.00375\ V}{1.375}\ \log.\ \frac{H}{H'}\cdot$$ where $\pi = 13568$ k = the weight of the metre cube of mercury. The other formulary is

$$\Pi\ (530^{\circ} + V - V').$$

the 7th of September, 1820 *. He employed for this purpose the therméliometre, described in the *Journal de Physique*, vol. lxxxvii, p. 256. He concludes from his observations, that the experimental heats are equal to those calculated on the hypothesis, that the effect of the sun-beams is proportional to the extent of surface of the portion of the disc of the sun uncovered; and of consequence that this hypothesis is the true one. Whence we may infer, that the disc of the sun is equally luminous over its whole extent, and that one part taken near the centre, emanates no more rays than an equal portion taken near the border of this disc. The opposite opinion was maintained by Bouguer; who said, that if we compare by the heliometer, (an instrument of his own invention), the sun's centre to a spot distant from it, by $\frac{3}{4}$ of the diameter, the quantities of the rays, which we receive, are in the ratio of 48 to 35. But this distinguished philosopher ingenuously admits, that his experiment contained difficulties which he had not surmounted, and that it, therefore, required verification. The equality of lustre now found by M. Flaugerghes, over all the parts of the disc of the sun, shews, moreover, he thinks the incorrectness of the geometrical hypothesis admitted by several philosophers, that the portions of the surface of a body, project in all directions an equal quantity of light; for on this supposition, the lustre of a spherical luminous body, ought to increase from the centre to the circumference, and become even infinite at the limb; which does not take place, as we may satisfy ourselves, in considering an iron bullet heated to whiteness; or a white paper globe exposed to the light of day. These spherical bodies will appear equally luminous over their whole surface. He also infers from his observations, that the opinion of M. Laplace, who pretends that the sun is sorrounded with an atmosphere such, that were it stripped of it, " this luminary would appear *twelve* times more brilliant," is void of foundation.

Some remarks on the temperature of mines will be found under GEOLOGY.

We shall conclude this subject for the present, by expressing our surprise at the ignorance of the state of English science, displayed in the following introduction to a Memoir of M. Despretz, read at the Institute, 29th November, 1819, and inserted in the 16th volume of the *Annales de Chimie et de Physique*, p. 105:—" It is admitted with Mr. Dalton, in all the works on physics, that on departing from the point of ebullition under the same pressure, or for greater generality, on departing from the point where the elastic forces are equal, the variation in the elastic force of the vapour for a

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† *Journal de Physique*, tom. 92, p. 435.

like number of degrees of the thermometer, is the same for all liquids." If M. Despretz will consult Mr. Dalton's *New System of Chemical Philosophy*, vol. i. p. 20, published upwards of thirteen years ago, he will find that the English philosopher there abandons that supposed relation, which otherwise is shewn in a Paper on Heat, published in the *Philosophical Transactions* for 1818, not to accord with experiment. We are thus saved the trouble of analyzing M. Despretz's Memoir.

III. SIMPLE BODIES.—In the number of *Gilbert's Annalen* for last May, Counsellor Giesse, Professor of Chemistry at Dorpat, communicates the account of a new metal, which, he thinks, he has extracted from the residuum left, on distilling to dryness different varieties of English sulphuric acid. One variety left out of 16 ounces, 9½ grains of a white residuum, which differed from the greater part of these residuums by the total absence of sulphate of lead. "The tints of colour," he says, "were surprising, which the residuum exhibited, when it was heated slowly and repeatedly in a platinum crucible over a lamp. From citron-yellow, it passed on cooling back again to white. At the second gradual heating, the mass appeared greenish yellow, then of a fine reddish yellow; and, on cooling, it passed once more through lemon-yellow to white. Strongly heated, a third time, it became sulphur-yellow, lemon-yellow, and reddish-yellow; it then fused with intumescence, and diffused a vapour, smelling of sulphuric acid. On becoming cold, the middle part was whitish; but the borders of a brownish hue. Though the complete series of experiments, which he meditated, were interrupted by a fit of sickness, yet he thinks sufficient have been made to satisfy him, that the above residuum contains a new metal, which must have come from the sulphur employed in manufacturing the acid.

The appearances characteristic of the new metallic substance, are produced by pouring on the residuum, after it has been thoroughly edulcorated with water, a little caustic alkaline ley. Immediately after the addition of the first drops, the white colour becomes yellow, which with heat assumes a golden tinge. He poured muriatic acid on the inspissated heated mass, so as speedily to dissolve it; he then diluted and filtered the solution, and poured in sulphuretted hydrogen water, which occasioned a precipitate of yellow-brown flocks, of the hydro-sulphuret of the metal. After these were separated, the liquid yielded, with ammonia, a hydro-sulphuret of iron in flocks, which speedily became dark green.

The metallic substance is susceptible, he says, of different degrees of oxidation; and becomes first yellow, then passes into a lively brown, bluish or dark grey; and it finally changes to white. At a certain stage of oxidizement it exhibits more the properties of an acid, than of a base; as it unites in this case easily with caustic alcali; but not with acids, such as the sulphuric and muriatic. The alcaline combinations of this new *metallic oxide* readily form double salts with acids; those with the above acids, are all crystallizable, and when dissolved by the aid of heat, yield, with ammonia, a precipitate of the metallic oxide.

These saline triple or quadruple combinations, do not always afford with sulphuretted hydrogen, a brownish-yellow precipitate; but occasionally give one of a bluish tinge, which communicates its colour to the whole liquid. The hydro-sulphurets of this metal, readily take fire with heat, and become white. A simple voltaic pair, consisting of zinc and platinum, throws down this new body from its solution, bluish-grey and with a perfect metallic lustre. The metal deposited at the platinum wire, dissolves in dilute nitric acid, with disengagement of gas. We must confess that the above account, containing the substance of the Professor's details, seems to us very inconclusive; and we should not be surprised to find that persulphate of iron is concerned in these phenomena.

The notice which was inserted in the *Journal of Science and the Arts*, vol. ii. p. 385, relative to the transudation of melted tin through cast iron, was slightly incorrect. M. Clement, the author of the experiment, had formed the cylinder of a pump, out of a copper tube, which being rather feeble, he wished to strengthen, by an outer case of cast iron. The interval between the two cylinders being filled with melted tin, this metal transuded on the *outer* surface of the cast iron, in the cotton-like efflorescence, which burned at the flame of a candle. This experiment shews the porosity of the cast iron employed, which we suspect must have been of inferior quality, or ill-founded.

IV. INORGANIC CHEMICAL COMPOUNDS.—The French chemists and artists have been much occupied of late in repeating and varying the experiments on the alloys of steel, which were made in our laboratory, and which are recorded in the ninth volume of this *Journal*. M. J. B. Boussingault, has made several, in the laboratory of the school of miners at Sainte-Etienne, particularly on the combination of silicium with platinum, and on the presence of silicium in steel. When platinum was placed in a crucible, lined with a paste composed of a mixture of powdered charcoal, and a little clay, (the *creuset brasqué* of the French chemists) it always fused, in a powerful air furnace, into a button; and the fusion was more easy, when the metal was covered with charcoal. It was observed that the platinum had increased a little in weight. The properties of platinum thus fused, are the following: it has a greyish-white appearance; it is scarcely affected by a knife, and with difficulty by a file; and has a specific gravity of 20.5. In the cold it yields a little under the hammer; but it presently cracks, and presents a granular fracture. Forged at a cherry-red, it crumbles in pieces; at a very dull red it flattens at first slightly, and then breaks. It is not softened in its temper at the blast of a forge; cemented with oxide of manganese for an hour, to remove its supposed carbon, it remained intractable. During the solution of the above platinum, no trace of charcoal was perceived; but when the process was somewhat advanced, a transparent jelly was observed to cover the fragments of metal, and to render its solution very difficult. After long action of the acid, with frequent agitation, the platinum was at last dissolved, and a white powder remained, which turned out to be silica, which M. Boussingault believes to have existed in a state of silicium, in the metal. He thinks it more than probable that this silicium comes from the wood-charcoal employed; for on burning it, the ashes yielded a notable quantity of silica, and from another experiment he satisfied himself that the silica was not derived from the crucible. Perhaps the silica, he says, may be in the deoxidated state in the charcoal. 5 grammes of pure platinum afforded 5.025 of the siliceous compound, 1 gramme of which yielded 0.010 of silica, being double the increase of weight on 1 gramme of platinum; thus indicating the conversion of 0.005 of silicium into 0.01 of silica, agreeably to Berzelius's proportions.

M. Boussingault next examined different varieties of French steel, as also a specimen from Monkland near Glasgow, made from Dannemora Swedish iron. His process consisted in dissolving the steel in sulphuric acid, diluted with six times its weight of water. The residuum, being well dried, and weighed, is then burned, and he infers the carbon from the loss. He concurs in opinion with Mr. Daniell, that these residuums, as well as those of cast iron, contain silicium; possibly in the state of a persilicate of iron, as the carbon is in the state of plumbago. It is to be remarked, that the residuums of the carbonaceous steels take fire in the platinum crucible before it is red hot; sometimes indeed the hand can endure the heat at which inflammation happens. What remained after the combustion, was digested with dilute muriatic acid, which dissolved the metallic oxides, and left the silica sensibly pure: it was calcined, and weighed while hot. In this process the manner of estimating the carbon was far from rigorous; but the chief object was the silica. The following is a table of the results;

	Iron.	Carbon.	Silicium.	Manganese & Copper.
Iron of Rive . .	99.825	traces	0.175	traces
Steel of cementation	99.325	0.450	0.225	traces
Cast steel . . .	99.442	0.333	0.225	traces
Monkland Steel . *Acier poule* . . .	99.375	0.500	0.125	traces

It would appear from these results, that in the cementation, at the same time that the iron combines with the carbon, it absorbs also a small quantity of silicium. If this fact, which at present is somewhat doubtful, were established by a great number of analyses of iron, before and after cementation, we could conceive the utility of certain substances which are added to the charcoal in the chests of cementation*. By exposing a pure iron, which contained no silica, to a melting heat, in contact with lime and clay, he obtained a portion of the steel of Clouet, which consisted, by analysis, of iron 99.2 silicium 0.8. From 100 parts, 1.6 parts of silica were obtained. There was no carbon in it. This steel has the property of hardening with water in the usual way, but it is not trusted to by workmen. Ten grammes of slender iron nails (which yielded no residuum after solution in sulphuric) were fused in a Hessian crucible. The brilliant melted button was more difficult to hammer and to file than the iron employed. It consisted, in 100 parts of iron 99.46, silicium 0.54; which last yielded 1.08 of silica.

We cannot therefore judge of the degree of fusion of iron when this operation is conducted in a Hessian crucible, since it seems to be sufficiently demonstrated, that at a high temperature, the iron reduces the silica, combines with the silicium, and forms a compound more fusible than the metal. The same thing happens to platinum, when it finds silicium ready reduced; and if it does not fuse in a Hessian crucible like iron, it is because from its slight affinity for oxygen, it cannot, like iron, decompose silica. In a *creuset brasqué*, steel melts first, then silicated platinum, and lastly manganese, in the same state of combination with the other metals. M. Boussingault infers that pure manganese is more difficult of fusion, than pure platinum.

On some Combinations of Gold, by M. J. Javal†. The object of this paper is to prove by experiment, that M. Pelletier's late notions concerning the equivalent number and combinations of gold are erroneous. M. Javal shews by varied researches on the oxide of gold, obtained by adding barytes to the chloride of that metal, that it consists of 11.909 oxygen united to 100 of metallic gold; a result which approaches nearly to 12.077, that obtained by Berzelius, by decomposing the neutral muriate of gold with mercury, and estimating from the quantity of gold obtained, of mercury which supplanted it, and rate of oxidation of mercury, the rate of oxidation of gold. M. Oberkampf had shewn that the sulphuret of gold obtained by precipitating this metal from the muriate, by sulphuretted hydrogen, consisted of 24.39 sulphur + 100 gold. Now $\frac{24.39}{2} = 12.195$, is very near the preceding number.

The yellow crystals which are obtained by evaporating the acid solution of gold treated with potash, was considered by M. Pelletier as a mere mixture of the two chlorides. By evaporating a mixture of the muriate of gold and muriate of potash, M. Javal obtained crystals of a fine golden yellow, having the form of elongated quadrangular prisms. These crystals did not lose their colour by washing; on the contrary, they continue to possess the colouring power in a very high degree. M. Javal afterwards analyzed these crystals, and found them composed as follows:

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* There is nothing added at Monkland to the charcoal; it may however contain a little silica.

† *Ann. de Chim. et de Phys.* Vol. xvii. p. 337.

Chloride of potassium	24.26	1 atom by Berzelius	25.21
——— of gold . .	68.64	2 atoms	68.71
Water	7.10	2 atoms	6.08
	100.00		100.00

Mr. Grouvelle, (not Granville, as misprinted in the last Number of our Journal, p. 174), has published in the *Annales de Chimie et de Physique* for last August, a paper on some compounds of chromium, and on several combinations, in which one of the elements enters only in a very small proportion. He says that bichromate of potash, when calcined strongly, melts, and passes to the state of neutral chromate, giving up the half of its acid which is decomposed, and leaves a crystallized oxide of chromium in spangles of a magnificent green colour. The neutral chromate obtained was analyzed by a solution of sulphurous acid, which converts it instantly into sulphate of potash, sulphate and sulphite of chronium. The metallic oxide was precipitated by ammonia, and the sulphate of potash evaporated.

M. Grouvelle says, that Vauquelin, by pouring sulphuric acid and potash into chromic acid, obtained a brown precipitate, which he regarded as an oxide of chromium, with more oxygen than the green oxide. It is not, says M. Grouvelle, an oxide, but a carbonate of chromium. Surely then it must have been a carbonate of potash which M. Vauquelin employed. It dissolves, he adds, with effervescence in dilute acids. When boiled in distilled water, it is decomposed, and we obtain green oxide, and carbonic acid. Consequently, we must avoid washing it with hot water.

We can also obtain this carbonate of chromium, by evaporating to dryness, a mixture of nitrate of ammonia, chromate, and carbonate of potash, or of muriate of ammonia, and an alkaline nitrate, carbonate, and chromate. The matter dried at a gentle heat blackens. It is to be re-dissolved in water, and a drop of water of ammonia is to be poured in, to separate a small quantity of carbonate of chromium, which, he believes, the nitrate of ammonia had rendered soluble. If it be too strongly heated, the excess of nitrate would re-produce some chromate. Here it is the protoxide of azote in the nascent state which decomposes the chromic acid; once gaseous, it has no longer this power. The mixture of nitre and sal-ammoniac acts as nitrate of ammonia, because a double decomposition is effected, in virtue of the facility with which the nitrate of ammonia is converted into gas. This double decomposition always happens when we heat, along with sal-ammoniac, the nitrate of a metal susceptible of forming a fixed chloride. In order to obtain the protoxide of azote, we may therefore use, instead of nitrate of ammonia, a mixture of nitre and sal-ammoniac, in the proportion of 3 of the former to 1 of the latter, leaving an excess of nitre, to prevent sublimation of the sal-ammoniac. By Dr. Wollaston's scale, 3 of nitre are equivalent to 1.58 of sal-ammoniac; so that M. Grouvelle's excess seems superfluously great; 2 of nitre to 1 of sal-ammoniac would be good proportions, and, to secure intimate mixture, they should be dissolved together in hot-water.

M. Grouvelle, in a short section on the chromites, offers nothing very definite or satisfactory. In treating of chromate of lead, he says, we may obtain a reddish chromate, by employing an alcaline chromate of potash; or, if we make use of a subacetate of lead, and a neutral chromate, boiling both together, a yellow precipitate is formed, which passes in a few seconds into a very brilliant orange-red. We may procure a still deeper tint, by boiling a little alcali with the red, or even with the yellow chromate of lead. He has analyzed, comparatively, the yellow chromate, the red, and the red ore of Siberia. All give exactly the same ratio between the acid and the oxide. They are neutral chromates, only the red chromate contains a small quantity of alcali, which appears to him about 1 or 1½ per cent. A red chromate may be obtained, by boiling together chromate of potash and litharge. From some experiments he infers, that the alcali is combined with the oxide of lead, and that this combination, united to the chromate of lead, gives rise to the red chromate, which thus contains a little more oxide than the neutral chromate. A few drops of dilute nitric acid deprive it immediately of its red colour, by dissolving the alcali and a little oxide of lead. He has found in the Siberian ore a little lime, but he is not sure whether it may not have been accidental. The above circumstances explain the fact known to painters, that the yellow chromate of lead mixed up with whiting, for painting walls, produces as great a body of colour as the same weight of the deep orange chromate.

M. Grouvelle precipitated magnesia, by caustic soda purified with alcohol, washed the precipitate till the water no longer reddened turmeric paper, treated it with nitric acid, calcined strongly and repeatedly the compound, then washed the magnesia, and obtained, finally, a nitrite which, with sulphuric acid, gave well-pronounced crystals of sulphate of soda. With potash the same result was obtained. We must take care to treat the nitrite by nitric acid, since being very alcaline it would not fuse on burning coals.

If we evaporate the washings of magnesia when the water ceases to be alcaline, reduced to about one-quarter it deposits a very notable quantity of magnesia, and then reddens turmeric paper. It is probable that the water decomposes the neutral combination formed, at the moment of precipitation, into a soluble compound, with excess of alcali, and an insoluble compound, with excess of base. The only means, therefore, of estimating exactly the quantity of magnesia which a solution contains, is to precipitate by phosphate of ammonia.

Magnesia, treated with barytes, presents the same phenomena, but they are easier to ascertain. M. G. acted on the compound of magnesia and barytes with a quantity of nitric acid, insufficient to dissolve the whole. The re-agents indicated both magnesia and barytes in the part dissolved, as well as in the part undissolved, which proves that there was really a combination, for otherwise the barytes would have been the first united with the acid. About 4 per cent. of barytes is thus associated with the magnesia. In like manner, oxide of copper retains a small portion of the potash, or barytes, employed to throw it down from its acid solutions.

In treating of the principles of combination at the commencement of this article, we promised to return to one of Berzelius's speculations. We shall now bestow a word or two on it. He is investigating the combinations of chromium. "Not having at my disposal," says he, "any metallic chromium, I have endeavoured to deduce the composition of its green oxide from its capacity for the acids. I dissolved the hydrate of chromium in muriatic acid; I evaporated this solution to dryness, and heated the muriate to a temperature sufficiently high to expel every trace of muriate of ammonia which might be present. The muriate thus heated appeared under the form of a red, pulverulent, and bulky mass; it dissolved very slowly, but without residuum in water. The solution was precipitated by ammonia, added in so small an excess that the liquid only re-acted feebly; it was thereafter digested with this excess to remove the muriatic acid precipitated with the oxide, in the form of a submuriate. The oxide thus obtained weighed 3.05 grains. The liquid, neutralized with nitric acid, and precipitated by nitrate of silver, produced 15.61 grains of muriate of silver. The experiment has then given, for 100 parts of muriatic acid 102.3 parts of oxide of chromium, which makes 28.5 parts of oxygen in 100 parts of green oxide. If the acid contains the metal combined with twice as much oxygen as in the green oxide, this last must contain 29.89 hundredths of its weight of oxygen; if, on the other hand, the ratio of the oxygen of the oxide were to that of the acid as 2 to 5, the oxide ought to contain only 20 parts of oxygen. If, finally, the oxygen of the oxide were to that of the acid in the ratio of 3 to 5, its composition would recede equally from the result obtained by experiment*."

As Dr. Wollaston's scale coincides for muriate of silver with Berzelius's atomic numbers, we shall employ it in the examination of the above passage; 15.61 muriate of silver contain 2.98 of what Berzelius still reckons muriatic acid, which were previously combined with the 3.05 of oxide of chromium. And 2.98 : 34.12: :3.05: :34.92, a number representing, by Berzelius's experiments, the equivalent of oxide of chromium, on Dr. Wollaston's scale. And 2.98 : 3.05: :100 : 102.3, precisely as M. Berzelius has stated. He adds, "which makes 28.5 parts of oxygen in 100 parts of green oxide;" hence, by proportion, in 3.05 parts 0.869, and in 3.49, or (to bring the atom to round numbers), in 3.5 parts, almost exactly 1 of oxygen. Thus the atom of chromium would appear to be 2.5, that of the green oxide 3.5, and that of the acid 5.5, the latter number of which is quite discordant with that deduced from his experiment on chromate of lead†, which gave 213.924 oxide of lead to 100 of acid; whence the latter becomes on the equivalent scale 6.54. One of the canons of combination laid down by Berzelius is, that 100 parts of muriatic acid neutralize a quantity of base, which contains 29.4 of oxygen, and from this canon his above calculation seems to be made. Now, this proportion is precisely what we find by sliding 100 on Dr. Wollaston's scale opposite to dry muriatic acid, for 29.4 is then opposite to oxygen. In like manner, by sliding 100 opposite to each other body on that instrument we may make as many canons as we please, but they all merge in the general system of equivalents. Dr. Thomson, in his account of the atomic theory‡, says, "According to Berzelius in order to saturate 100 parts of muriatic acid, a metal must be combined with 42 parts of oxygen." We shall not attempt to unravel this atomic knot. While we entertain the highest respect towards the Swedish chemist for his unwearied and valuable researches, we cannot help thinking his manner of viewing chemical proportions to be unaccountably perplexed and defective. We can more easily understand why his partial mode of notation should be preferred by many writers in France to the universal system, as taught by Mr. Dalton, and so clearly exhibited on Dr. Wollaston's scale, for the same reason that vaccination was so ill received in that country,—because it was discovered by an Englishman.

We are lately indebted to Professor Berzelius also, for a pretty long memoir, on the mode of analyzing the ores of nickel, and on a new combination of nickel with arsenic and sulphur. We shall reserve an account of this for our next Number, in order to connect it with some more extended observations on analysis, than we have room for in the present. Mr. Robiquet's observations on the memoir of M. Berzelius, relative to the composition of the triple prussiates or hydrocyanates, concerns a subject too intricate and important to be lightly passed over. We shall, therefore, bring it under review in the next Number.

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* *Annales de Chimie et de Physique.* Tom. xvii. p. 10.

† *Ibid.* Tom. xvii. p. 7. ‡ *Annals of Phil.* ii. 39.

V. ORGANIC COMPOUNDS.—Under this head we shall consider all chemical combinations, which directly or indirectly result from vegetable and animal organization.

M. Dive, apothecary, of Mont-de-Marsan, has formed prussiate of potash by calcining in a covered crucible a mixture of 64 grammes of the dry powder of crude tartar, and 8 grammes of pulverized sal ammoniac. He considers that the nascent carbon of the tartar, being presented to the nascent ammonia, disengaged from the muriate by the potash, acts so as to form cyanogen; and that this action is favoured by the temperature, which weakens the combination of the hydrogen and azote in the ammonia. The latter element being, in every point of the mass, in immediate contact with particles of carbon, easily unites to it, in the requisite proportions for forming cyanogen, which is immediately fixed by the potash. The same gentleman finds that a current of carbonic acid partially decomposes the neutral tartrate of potash; and he ascribes to this cause the formation of the bitartrate in the juice of the grape during its fermentation. Accordingly, on mixing neutral tartrate with fermentable materials, he found cream of tartar in the fermented liquor *.

In a report made to the Institute, 15th January last, by M. M. Thenard and Berthollet, on M. Chevreul's 8th memoir on Fat Bodies, we find the following results of their decomposition by ignited oxide of copper.

1. That the fat of man and the sow contain nearly the same proportions of elements; that mutton suet contains more carbon and hydrogen; and that in the three fats the carbon is to the hydrogen, by volume, nearly as 10 to 18; which approaches to the constitution of percarburetted hydrogen.

2. That the stearines contain less oxygen, and more carbon and hydrogen than the elaïnes; and that the ratio of the carbon to the hydrogen in the stearines is 10 to 18, whilst it is a little less in the elaïnes.

3. That the sum of the weight of the saponified fat, and the

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* *Journ. de Pharm.* Oct. 1821. p. 487.

sweet principle, which are the result of saponification of different species of fats, is greater than the weight of the fat employed. If we recollect that the saponification takes place *in vacuo*, without any other substance than the fat, the potash, and the water, and that there is no disengagement of hydrogen and oxygen, we must necessarily admit the fixation of water in one of the products of the saponification. Comparative tables of all the products and of their elements, whether in weight or in volume, establish these consequences; and the author observes, that, in the acidified fats, the hydrogen is to the carbon in the ratio of the elements of olefiant gas. When we heat gently with massicot (yellow oxide of lead) the margaric and oleic acids, and consequently the acidified fats which are formed from these acids, there is disengaged a quantity of water, which, as the author shews, is formed of the combination of the hydrogen of these acids with the oxygen of the massicot, or which is disengaged simply by the combination of the massicot with the dry acids. The author adopts the latter opinion. He exhibits, in comparative tables, the elements of natural fats, and the products of their saponification, both of the margaric and oleic acid. There results from their comparison :

1. That in the three margaric acids which he has examined, the carbon is to the hydrogen perceptibly in the proportion of the elements of olefiant gas.

2. That the oxygen of the margaric acid of the sheep, is to the oxygen of the margaric acids of man and the hog, nearly as 1 to 1½. From this observation the author proposes to name the margaric acid of mutton suet, *margarous* acid.

3. That in the oleic acids of man and the hog, there is more oxygen than in the margaric acid; and that in that of sheep there is more than in *margarous* acid.

It is remarkable that the composition of oleic acid is represented by olefiant gas + oxide of carbon.

When potash or any other alcaline base acts on the fats which have been examined, the greater part of their carbon and hydrogen, in a ratio very near to that of olefiant gas, retains a portion of oxygen to constitute the margaric and oleic acids, whilst the rest of the elements of the fat, that is to say, of the carbon, hydrogen, and a quantity of oxygen, which seems less by one-half than what would be necessary to burn the hydrogen, form the sweet principle, by fixing probably a portion of water.

The elementary analysis of cholesterine shews, by its excess of carbon, the reason why this compound produces no margaric acid, when it is exposed to the action of alcalies.

At page 389, vol. x. of this *Journal*, we have given some account of the chemical researches of M. M. Pelletier and Caventou, on cinchonine and quinine. But the mode of obtaining these new vegetable bases was omitted; we shall now lay it before our readers.

Two kilogrammes (about 4½ pounds avoird.) of pale cinchona, bruised, were acted on by 6 kilogrammes (about 13½ libs.) of strong alcohol. This operation was repeated four times. The alcoholic tinctures were united, and distilled, to withdraw the alcohol. Care was taken to add 2 kilogrammes of distilled water, in order that the matter dissolved in the alcohol should be protected from the immediate action of the heat, after the separation of the alcohol. This substance, received on a filter, which allowed the aqueous liquid to pass through, was of a reddish colour, and a resinous aspect. In this state it was washed on the filter with water rendered slightly alcaline by potash. The liquor which had passed through the filter served for the first washing, after having been previously alcalized. After several days of edulcoration, the alcaline liquids passing limpid and colourless, the matter left on the filter was washed with a pretty considerable body of distilled water. The substance was then of a greenish-white, very fusible, soluble in alcohol, and crystallizable. It was the cinchonin of Dr. Gomez. It possessed in this state some of the characters of resinous substances; but, on dissolving it in an acid very much diluted with water, it deposited a considerable quantity of a fat matter of a green colour, which had all the characters of the green fat matter obtained for the first time by M. Lauber, by the direct action of ether on cinchona. We may remark, that if too strong an acid be employed, a great quantity of the fat matter would remain in solution in the liquor, and the cinchonine, which we should subsequently obtain, would be polluted with it.

The acid liquor (dilute muriatic acid was employed) was of a golden-yellow. Evaporated, it yielded crystals soluble in alcohol, and in water. Its taste was very bitter. It fell down readily with alcaline solutions; the gallates and alcaline oxalates occasioned precipitates soluble in alcohol. The above solution was treated with pure magnesia at a moderate heat. The mixture, after thorough cooling, was thrown upon a filter, and the magnesian precipitate was washed with water. The first washings were yellow; but they soon became colourless. The magnesian precipitate, sufficiently washed and dried at the *balneum mariæ*, was treated three several times with alcohol of 40° (0.817). The alcoholic liquors, very bitter and slightly yellowish, gave by evaporation crystals in needles, of a dirty white. These crystals, re-dissolved in alcohol, and made again to crystallize, gave a very white and brilliant crystalline matter. We may also obtain very white crystals by washing them in the cold with a little sulphuric ether. These crystals are pure cinchonine.

Quinine is obtained from yellow Peruvian bark, by employing the different processes indicated for the extraction of cinchonine. In case of a natural or artificial mixture of cinchonine and quinine, crystallization and ether may serve to separate these two bodies. Quinine is very soluble in ether, and does not crystallize *.

M. J. Voreton, of Grenoble, has given the following as an improved process †. He takes 5 kilogrammes of yellow bark of good quality, reduced to coarse powder. These are infused in water acidulated with about one hundredth of its weight of muriatic acid. After macerating 24 hours, he expresses strongly, and treats the matter with new acidulated water till the bark gives out no more bitterness. He then filters, and treats the united infusions with an excess of pure magnesia. This mixture is boiled an instant, and then allowed to cool. The remainder of the process is exactly conformable to that already known. Thus we filter, wash the magnesian precipitate with cold water, dry it, and treat it with alcohol. We obtain the quinine by distilling off the first portions of the alcohol, and evaporating the rest at a very low temperature.

By this process, more simple than that formerly published, since we avoid the preparation of the alcoholic extract, he has obtained from 5 kilogrammes of bark about 70 grammes of quinine, instead of 45 or 50, which was all he could obtain by the former process. This arises, he believes, from the muriatic acid dissolving more easily all the quinine, when this is not enveloped by the fat or resinous matter of the alcoholic extract.

But the most economical and productive process is that of M. Henry, fils ‡. A kilogramme of yellow bark, (cinchona cordifolia) reduced into a pretty fine powder, was acted on twice with heat, by a dilute sulphuric acid, consisting of 50 or 60 grammes diluted with 8 kilogrammes of water for each time. The filtered decoctions were very bitter, and had a reddish colour, which assumed on cooling a yellowish tint. To discolour (blanch) these liquors, and saturate the acid, he employed pulverized quick-lime instead of the more expensive magnesia. The liquors, entirely deprived of colour, were passed through a cloth, and the precipitate which formed, was washed with a small quantity of water, to separate the excess of lime. The deposit (on the cloth,) well drained and almost completely deprived of moisture for twelve hours, after having been put three successive times to digest in alcohol of 36° (0.837), furnished by distilling off the alcoholic liquid, a brown viscid matter, becoming brittle on cooling. It was acted on with water sharpened with sulphuric acid, and the refrigerated liquor afforded nearly 32 grammes of white

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* *Annales de Chimie et de Phys.* xv. p. 289.

† *Ibid.* xvii. p. 439.

‡ *Journ. de Pharm.* July 1821. p. 296.

silky crystals, entirely soluble in alcohol, scarcely soluble in cold water, but more in boiling water, particularly if this was slightly acidulated. They consist of pure sulphate of quinina. Messrs. Pelletier and Caventou, while they approve of the above process, consider the product as overrated, probably from M. Henry not having sufficiently dried the sulphate obtained. The pure sulphate of cinchonina ought to be brilliant, crystallized in parallelipeds, very hard, and of a glassy white; the sulphate of quinina is on the contrary of a dull white, silky and flexible; both should be soluble in alcohol and should burn without leaving any residuum*.

In the *Journal de Pharmacie* for last September, M. Baup, of Vevai, describes the crystallized sulphate and supersulphate of quinina, as consisting, the first of—

Quinina . .	1	atom	45
Sulphuric acid . .	1	—	5
Water . . .	4	—	4.5
			54.5

The supersulphate consists of—

Quinina . .	1	atom	45
Acid . . .	2	—	10
Water . .	16	—	18
			73

The crystalline form of cinchonina is a rhomboidal prism, of 108° and 72°, terminated by a bevelment.

M.M. Pinel, Thenard, and Hallé, in a report recently made to the Academy of Sciences, on a Memoir of M. Choulant, entitled, " Observations on the employment of the Sulphates of Quinina and Cinchonia in Intermittent Fevers," give the following conclusions: The number of cases reported by M. Choulant is 14; in 10 of them the cessation of the fever was due to the sulphate of quinina, which occurred immediately after either the first or the second dose; and, in the latter case, the paroxysm that followed the first was considerably milder; the effectual doses varied from 6 to 12 grains. The reporters also state, that M. Double has administered the sulphate of quinina in tertian and quartan agues, with immediate and complete success, and almost always after the first doses. The small bulk of the medicine renders its administration easy in many cases in which the bark in substance would be inadmissible. The red or yellow bark seems to yield the most febrifuge quinina.

VI. ANALYSIS AND APPARATUS.—In the last Number of our Journal, p. 169, we have described M. Berthier's mode of analyzing minerals which contain alcaline matter. The following Table of results seems important:

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* *Journ de Pharm.* July, 1821. p. 305.

Constituents.	Adularia.	Petrosilex.	Pumice.	Domite.	Domite.	Vitreous Lava.
Silica . . .	0.6420	0.7520	0.7000	0.6550	0.6100	0.6440
Alumina . .	0.1840	0.1500	0.1600	0.2000	0.1920	0.1564
Potash . . .	0.1695	0.0340	0.0650	0.0910	0.1150	0.0540
Lime . . .	trace	0.0120	0.0250	0.0220	. . .	0.0120
Magnesia . .	. . .	0.0240	. . .	. . .	0.0160	0.0120
Oxide of iron .	. . .	. . .	0.0050	0.0300	0.0420	0.0430
Water . . .	. . .	0.0150	0.0300	. . .	0.0200	0.0710
	0.9955	0.9870	0.9850	0.9980	0.9950	0.9924

The domites from Puy de Dome, and the vitreous lava, have been called feld-spar lavas.

1. *Analysis of the crystallized Calamine of Limbourgh* (Electric oxide of Zinc, Haüy,) by Professor Berzelius. (From Schweigger's Journal for last December.)

Mr. Smithson first distinguished several varieties of calamine which have since formed a single species. That now under consideration was by him found to consist of, silica, 25; oxide of zinc, 68.3; and water, 4.2=97.5.

The silica and oxide are in the equivalent proportions, but the water is in this respect anomalous. With a view of settling this point, M. Berzelius undertook a careful analysis of the minerals. On redissolving in ammonia, the precipitate by subcarbonate of soda, from the sulphuric acid solution, he separated completely the oxide of zinc from the lead, and found of the latter 0.003 parts. The following is his result:

Silica	24.9	or	26.23
Oxide of zinc . .	66.84		66.37
Water	7.46		7.40
Carbonic acid . .	0.45		100.00
Oxide of lead . .	0.28		
	99.93		

Representing this compound by the formula $2\ \mathrm{zn}.S+\mathrm{aq}.$, and subtracting the carbonate of zinc.

2. *Analysis of an arsenical pyrites, from Zinnwald*, by Dr. du Menil; specific gravity, 6.064; constituents, iron 42.44, arsenic 52.12, sulphur 2.4.

3. A variety of *malacolite* from Norway (sahlite,) sp. gr. 3.1, analyzed by Count Trolle, gave silica 57.4, lime 23.1, magnesia 16.74, loss 2.76, in 100 parts. He considers it as represented by the formula $C\,S^2 + M\,S^2$; that is, a bi-silicate of lime associated with a bi-silicate of magnesia.

4. *Analyses of some northern minerals*, by C. d'Ohsson, Swedish ambassador at the Hague. They were undertaken at the invitation of Berzelius, and verified by him.

Garnet of Broddbo, sp. gr. 4.25. It fuses into a black globule before the blow-pipe. It consists of silica 39, alumina 14.3, protoxide of iron 15.44, protoxide of manganese 27.9, tungstate of zinc 0.5, silicate of zinc 0.5, loss 2.36 in 100. He represents its atomic constitution, on Berzelius's plan, by $F\,S^2 + 2\,Mg\,S + 2\,A\,S$.; that is, 1 atom of bi-silicate of iron, +2 atoms silicate of manganese, +2 atoms silicate of alumina.

Malacolite of Bjornmyresveden, sp. gr. 3.331. It consists of silica 57.28, lime 24.88, magnesia 9.12, protoxide of iron 6.04, protoxide of manganese 0.72, loss 1.96; or, $M\,S^2 + 2\,C\,S^3$, viewing the iron and manganese as accidental associates.

A new mineral from Pargas, called *Chondrodite* (granular,) colour wine-yellow, occurs in grains about the size of a pinhead, fracture small conchoidal, lustre between waxy and vitreous, scratches glass, sp. gr. 3.18. Dissolves in neither nitric nor muriatic acid; infusible before the blow-pipe, but becomes darker coloured; with soda it fuses into a bright green slag. With borax it readily melts, with intumescence, into a bright greenish-yellow glass. Its constituents are, silica 38.0, magnesia 54, oxide of iron 5.1, alumina 1.5, potash 0.86, manganese a trace, loss 0.54. Viewing the iron and the latter articles as unessential, M. d'Ohsson thinks its constitution may be represented by the simple formula $M\,S$, a silicate of magnesia. It occurs near Pargas, in Finland, interspersed in granular limestone.—Schweigger's *Journal*, Dec. 1820.

Examination of a meteoric stone which fell in Courland, in the circle of Dünaberg, on the 12th July, 1820, by Theodore Von Grotthus. Between five and six o'clock in the evening, a fire-ball apparently as large as the full moon, was seen moving slowly from south to north. It seemed to burn with a reddish flame, which also followed it, like the train of a comet, leaving in the distance spiral clouds, which moved very slowly, and then melted in air. After this meteor had described in the heavens an arch of nearly 100°, it became extinguished. In less than a minute afterwards there resounded from the region where the fire-ball had vanished, a noise resembling, first, three rapid discharges of great guns; then a fire of musketry; and finally, a continuous rolling like thunder peals. At the same time, a stone fell from the air, 3½ German miles from the country palace of

Lixna, about fifty paces from two husbandmen, who were working in the field, and who were greatly astonished. About four wersts distant, in the presence of six labourers who were mowing hay on the banks of the Kolupschen lake, there fell at the same time, with a frightful hissing sound, a large body into the water dashing its spray several fathoms into the air. Also at a third place, three wersts in the opposite direction, there was observed to fall from the air into the river Dubna, something which rendered its waters muddy for nearly an hour. The stone which fell in presence of the two husbandmen, penetrated a foot and a half into a dense, dry, clayey loam. It was so hot that the men when they tried to touch it, after recovering from their surprise, burned their hands. There was also a smell of gunpowder diffused round it. Its shape, when entire, resembled a rounded anvil, of which the narrow end was undermost. It weighed altogether about forty pounds. Its external characters are described by M. Grotthus, and resemble those of other meteoric stones. Its composition, according to his elaborate analysis, is as follows : Iron 26, nickel 2, sulphur 3.5, (which bodies he regards as forming 31.5 of a sulphuret of iron and nickel), silica 33.2, protoxide of iron 22, magnesia 10.8, alumina 1.3, metallic chromium 6.7, lime 0.5, manganese a trace. His mode of analysis is somewhat peculiar, and we shall perhaps advert to it in our next Number. He thinks the proportions of chromium and manganese are very difficult to determine, when present in small quantity, unless we remove the greater part of the iron by muriatic acid, before fusing the powder with caustic potash. In this way also the mass yields more readily to the alcali. He believes the chromium to have existed in this meteorolite in the metallic state, because had it been oxidized, it would have dissolved in the muriatic acid.

The following mode of analyzing copper pyrites by Professor Dobereiner seems ingenious. He transmits over it a stream of chlorine, dried by passing previously over muriate of lime. The pyrites is entirely decomposed, and chlorides of sulphur, iron, and copper result. On heating this mixture with a spirit lamp, the first chloride distils over into a separate vessel, the second sublimes in brilliant flakes into the upper part of the retort, or matrass, and the cupreous proto-chloride remains at the bottom in the form of a cinnamon-brown semi-fused mass*. There is much resemblance between this method and Berzelius's for analyzing the ores of nickel, of which an account will be given in our next Number. We were surprised however to perceive M. Dobereiner describing, as a new contrivance of his own, the pneumato-mercurial apparatus, so long ago invented by Mr. Pepys; with which he and Mr. Allen performed their celebrated experiments on carbonic acid and respiration.

Meteorolite in France.—On the 15th of June last, about three o'clock P.M., while the sky was serene, a very considerable globe of fire was seen in the air, which seemed to descend rapidly in a straight line. Immediately thereafter a loud rattling noise and a detonation were heard, and a stone weighing 92 kilogrammes (203 libs. avoird.) fell in the Commune of Juvenas, department of Ardèche. According to the report of a villager its appearance was announced by two strong explosions, like two discharges of a cannon, and followed by a rumbling noise, which spread consternation among the inhabitants. The stone had sunk 18 decimetres into the ground, that is, nearly 6 feet. It is also said, that some smaller stones fell on the occasion. The fragment of the great one, which was sent to the Academy of Sciences, has the usual external characters of meteorolites.

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* Gilbert's *Annalen* for 1821, Part 4.

II. APPLICATIONS OF CHEMISTRY.—Under quinina we have already pointed out the medical applications of this substance. M. Sexullas in a memoir, of which there is a copious abstract in the *Journal de Pharmacie* for September, shews, that all the antimonial preparations used in medicine, except carefully crystallized tartar emetic, contain more or less arsenic, which originally combined with the antimony in the ore, continues pertinaciously associated with it through all its modifications. He also proves in the same paper, that a very powerful pyrophorus is obtained, by treating tartar emetic in the same way as the mixture of alum and flour is treated for making the pyrophorus of Homberg. The curious details on this subject are reserved for our next *Journal*.

Of the severity of the French *police Pharmaco-legale*, we may judge, when M. L—, a respectable apothecary of Verdun has been recently fined 3,000 francs, for selling sulphuric acid to a woman, who poisoned herself with it.

The experiments of M. Magendie having shewn that the salt, extracted long ago from opium by Derosnes, and which is improperly called narcotine, produces a stupor differing from real sleep, and acts on dogs as a poison in small doses, M. Robiquet conceived that he might render opium a more soothing medicine by depriving it of this irritating and pernicious principle. Accordingly M. Robiquet has prepared an extract of opium on good chemical principles, which has produced happy effects. He macerates the opium, cut into small pieces, in water, as if to obtain the aqueous extract; he filters and evaporates to the consistence of a thick syrup, and treats this extract with ether, agitating very frequently in a convenient vessel. He decants the ethereous tincture. This, when once separated, is submitted to distillation to recover the ether. He repeats the operation as long as he obtains crystals of *narcotine*. When the ether has no longer any action on the extract, he evaporates the solution, and the opium is prepared. As the same ether may be employed to prepare a new extract of opium, the operation is by no means so costly as would at first sight appear.

Dyeing. "Simple and easy method of clearing from their tawny pigment, dyeing infusions made with Brazil woods of inferior quality, and of substituting them with success, for true Fernambuco, by Dr. Dingler, manufacturing chemist at Augsbourg." The watery infusions of these poorer dye woods are to be evaporated, till from 4 kilogrammes of wood there remain only 12 or 15 of liquid. When this liquid is cooled, we must pour into it, after 12 or 18 hours, 2 kilogrammes of skimmed milk. After stirring this mixture well, we boil it for a few minutes, then pass it through a piece of thick flannel. The tawny colour will be then seen to attach itself to the caseous part of the milk, which spontaneously precipitates from this decoction, without occasioning the least loss in the quantity of red colour. The remaining red is of a pure tint. Perhaps this process might be useful with madders, which consist of the same two colours. Two kilogrammes of milk are sufficient for 6 or 8 kilogrammes of young woods *.

Paste for dressing Webs.—It is well known that weavers are obliged to work in damp shops, to prevent the dressing of the web from drying and hardening. M. Dubue has read lately a Memoir before the Academy of Sciences of Rouen, on the subject of pastes, &c., in which he shews that a very minute addition of muriate of lime to them, renders them so retentive or absorbent of moisture, that webs dressed with such pastes, may be wove in the upper and drier chambers of a house, as well as in the lower and ill-aired with the usual dressing. The plan is undoubtedly judicious. Muriate of lime may be had at a very trifling expense from those apothecaries who prepare water of ammonia. The waste whitening steep of the bleacher is merely a solution of muriate of lime.

Agriculture.—Professor Gazzeri, of Florence, has lately published an Essay on Manures, and their most useful mode of employment in Agriculture. To the English farmer, acquainted with the agricultural practice of Mr. Coke, and the Treatise of Sir H. Davy, the above work offers little novelty; but its precepts must be very useful in Italy; and, indeed, his experiments seem so judicious, as to deserve a brief notice in this place. "I shall demonstrate," says he, "both by reasoning and facts, that the previous fermentation of manures is not well fitted to make their substances enter into plants during vegetation, if that fermentation takes place at a distance from the soil which the manures are meant to fertilize; but that, on the contrary, the application of manures in their entire chemical state is advantageous, especially if they have been subjected to mechanical division, (such as chopping of the straw, &c.) To my great surprise, experiments have convinced me of two things; first, that the loss of useful matter suffered during fermentation is much greater than I could have conceived; and, secondly, that if the solubility of the residuum be ever so little increased, the quality of that residuum is still more deteriorated." He placed in a copper pot a mass of horse-dung, weighing 40½ libs, (of 12 ounces each), which he surrounded with coarse cloth, and a large body of straw, to screen it from the action of air and light; and it was placed in a close and covered situation, circumstances all unfavourable to fermentation. It consisted then of

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* *Journal Polytechnique d'Augsbourg.*

Water	70.37
Fibrous matter	15.17
Attenuated stercoraceous matter .	11.12
Soluble parts	2.34
	100.00

At the end of two months it had lost 9½ pounds of its weight. Its proportional composition was now, water 68, fibres 16, attenuated organic matter 11, soluble matter 4. At the end of another month it had lost 3 pounds more. Some time after this, at the beginning of July, he opened the pot, to give the manure free access to the air, and on the 18th of that month, being 4 from the commencement, its weight had diminished to 22 libs. 3 ounces. The proportion of its constituents, was, however, much the same. Thus, though placed in circumstances the most favourable to its preservation, it had lost in four months more than one half of its substance. He found that the richer animalized matter was that soonest dissipated. Thus the manures lose their azote, an element essential to the growth of wheat, as it forms a constituent of its gluten, without which panary fermentation does not take place. M. de Saussure's experiments prove that these grains do not derive their azote from the atmosphere. His experiments on the dung of fowls, cows, *&c.* offer similar results. The decomposition of cow dung was found to be much slower than that of horses. He was surprised to find its decomposition accelerated by being mixed with straw. But the freer access of air in this case will account for the difference. In great dung hills, the author says, that decomposition goes on more rapidly, and is more wasteful. The attenuation of the fibrous portion is thus very dearly bought.

Another result of the author's experiments which we think interesting is, that fresh vegetable matter, such as chopped straw, decomposes more rapidly into soluble manure, when mixed with soil in which plants are growing, than when left in soil in which no vegetation is going on. It also disappears sooner, being dissolved by the living powers of the vegetables, and carried into their circulation.

We were not a little amused with the contrast in sense and science, between the work of the Italian Professor, and an Official Report, signed by Count Dubois, Yvart, and Hericart de Thury, Members of the Commission of Manures of Paris, made to the Royal and Central Society of Agriculture, on a new Manure proposed under the name of the Alkalino-Vegetable *Poudrettes.* These gallant gentlemen extol one of their countrywomen, Madame Vibert Dubout, patentee of the Poudrettes, for her discovery and indefatigable practice of the following operations :—" After allowing the consistent ordure to subside, she draws off the urinous liquid into a basin, and leaves it in repose for fifteen or twenty days, to allow time for the alkaline and saline elaborations to be naturally formed. She then slakes or diffuses quicklime in the urines, in the proportion of a fifth part of the whole. At this moment the lady feels herself exposed to an infectious and insupportable odour, from the escape of various gases. At the end of eight days the slaked lime forms a fair or yellowish paste, soft and soapy to the touch, which diffuses the odour of violets. This paste is removed, spread on a spacious and well-aired area, covered with a layer thrice its thickness of the consistent ordure, which had been meanwhile dried and drained in the upper basins. On these two primary beds others are formed in alternate succession, taking care to leave from space to space layers of the thick matter in mutual contact, in order to facilitate their fermentation. In a shorter or longer time, according to the state of the weather, there arises in the mass a fermentation, more or less active, which rises, becomes puffy, and speedily confounds the different strata of the alternate matters. When the fermentation has ceased the great masses must be cut down, intimately mixed together, piled up anew, and left to become thoroughly dry. Finally, at the end of a certain time, a mixture absolutely inodorous is obtained, which is to be reduced into powder, by the ordinary processes, and which forms the new patent *Poudrette* of *Madame Vibert Dubout.*" We shall not stop to enumerate the marvellous virtues ascribed to this spoiled manure, by the learned commissioners. That a lady should contrive, execute, and describe so abominable a process, is to us in England not a little surprising ; but that an operation so unscientific, destructive, and absurd, should be recently held forth to admiration, in the *Annales de l'Agriculture Française*, is curious in the extreme, and must give the world an odd opinion of French husbandry.

III. PHYSICS. In our next Number we shall endeavour to present our readers with a view of the discussions on Light, which now occupy so keenly the attention of some leading members of the Academy of Sciences.

Magnetism. Coulomb determined the law of magnetic attractions to be inversely as the square of the distance, by making a magnetic needle vibrate, at different distances from the magnetic pole of a bar, and counting at each station, the number of vibrations. Professor Hansteen of Christiana* has lately applied the same method, to discover if the absolute magnetic force of the earth be an uniform or variable quantity, for any particular place on its surface. He suspends a magnetized cylindric rod, 2 inches and $\frac{3}{4}$ long, and $\frac{3}{40}$ of an inch diameter, by a single thread of a silk cocoon, depending from the top of a brass tube, fixed in the centre of the lid of a brass box. On either side of this vertical tube, the lid of the box is formed of glass, to permit the graduated arcs at the bottom to be seen. This mode of suspension has also, we observe, been adopted by M. Arago, for his very delicate needle mounted on the Royal Observatory of Paris; and, it is considered by him as infinitely superior to the suspension on a point, whose form is liable continually to change by the effect of friction. Professor Hansteen sets his needle in movement by presenting to one of its poles a piece of iron, which is immediately withdrawn. He then counts, by a chronometer, the period of every ten vibrations up to 360; and he compares each equal portion of the whole to find the mean period. Thus he takes the difference between the end of the first, and the end of the 300th; the end of the second, and that of the 310th; and so on, till that of the 60th and the 360th. Did not the resistance of the air gradually diminish the arc of the vibrations (till towards the close, it becomes only two degrees), the above measures would be equal; but in consequence of this resistance, the last measure of the time is found to be about $\frac{8}{10}$ of a second less than the first. The longest period of 300 oscillations observed by M. Hansteen was 813.6″; this time he considers as corresponding to his minimum or zero of magnetic force. In fact, this observation was made during an aurora borealis. But the force of the magnetic power is inversely proportional to the squares of the times of similar vibrations of the same magnetic needle; and, if T T′ be in two experiments, the observed times of equal vibrations of the same needle, and if I I′ denote the corresponding forces of the magnetic power; then $I : I' :: T'^2 ; T^2$; and thus

$$I : 1 :: 813.6'' ; T^2,\ \text{hence}$$

in this case

$$I = \left(\frac{813.6}{T}\right)^2$$

From numerous observations of the above kind, which the

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[*] In a letter to Mr. Rümker lately of Hamburg, of which an abstract is given by Professor Gilbert, in his Annals for last July.

professor made during the course of the year, he has calculated the mean monthly intensity of the magnetic force. The following is a table of the results :

Hour of Observation.	December 1819.	March 1820.	Hour of Observation.	April 1820.	May 1820.
8 M.	1.01931	1.01095	8 M.	1.00717	1.00582
10	.01902	.01010	10½	.00625	.00548
12	.01915	.01023	4 A.	.00879	.00849
2		.01136	7	.00966	.00844
4	0.01916	.01147	10½	1.00903	1.00740
6	0.01929	.01163	Mean	1.00818	1.00713
9		.01142			
10	0.01732	.01063			
Mean	0.01912	1.01081			
Hour of Observation.	**June 1820.**	**July 1820.**	**August 1820.**	**Sept. 1820.**	**October 1820.**
8 M.	1.00407	1.00277	1.03309	1.00560	1.00886
10½	397	235	335	508	800
4 A.	647	461	543	708	909
7	700	500	570	711	953
10½	1.00665	1.00548	1.00555	1.00715	1.00953
Mean	1.00563	1.00404	1.00468	1.00640	1.00900

From these observations it follows ; first, that these is a daily variation in the strength of the magnetic power; the *minimum* occurring between 10 to 11 A.M, the *maximum* between 4 and 5 P.M. Second, that the magnetic power is subject also to a yearly variation in its magnitude, as appears from the mean of each month ; its amount is manifestly greater in winter, while the sun is in perigee, than in the opposite season ; in December and June the greatest monthly variations also take place ; and the *minimum* variations occur in the two months of spring and autumn, when the sun is at his mean distance from the earth. The greatest daily variation of the magnetic power is in summer ; the least in winter. The maximum difference of the annual variation is 0.0359.

If we denote by F the total magnetic power, by H that portion of this power which acts on a horizontal needle, and by *n* the magnetic inclination, then $H = F +^{2} \cos. n$. Hence H may be variable, when the whole magnetic force F is constant, provided the magnetic inclination be variable. But M. Hansteen imagines, from researches which he made in 1820, with a dipping needle by Dollond, that he found the inclination to be in summer about 15′ greater than in winter; and from 4′ to 5′ greater in the forenoon than in the afternoon; which would accord with the foregoing observations *.

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* On this subject M. Arago judiciously observes, that from Mr. Gilpin's observations, whose accuracy is acknowledged by philosophers, there appears to be no appreciable diurnal variation in the dip, and its monthly

During an aurora borealis, he made a series of observations from noon to noon without intermission, which evidently shewed that this meteor displays an enfeebling operation on the magnet; proving in this respect, the connexion between electricity and magnetism *. From analogous researches he has been led to infer, that the magnetic intensity is always impaired when the moon passes through the equator.

During a residence of a few days last year in Copenhagen, M. Hansteen lodged in the round tower, which serves as an observatory. As he was repeating here the above-described observations on the vibrations of his horizontal needle, he found, to his great astonishment, that for 300 vibrations not less than 836.57″ were required: and in a garden contiguous to the tower, only 779″ for the same number of oscillations. This tower is 126 feet high, with very thick walls, and has, built in its centre or axis, a hollow cylinder, round which the stair winds in 7 convolutions. After he had made several observations below, which shewed that the mean period of 360 vibrations was 787″, he returned to the tower, and found the following duration of 300 oscillations; first, aloft on the tower; then at regular stages of descent to the bottom:

Top.	1st platform	3.	4½.	6½.	bottom
842.37	836.57	837.3	834.43	804.07	813.0

Not a little astonished at these results, he continued his observations, and deduced at last the general result, that at the under end of every perpendicular body the magnetic needle oscillates quicker when it is placed northward of the body, or when it presents its south pole to it; but that it oscillates more slowly when placed southward of the body, so as to present its north pole to it. And inversely, it was found, that, at the upper extremity of the vertical body, the needle vibrated always slower on the north side, and quicker on the south side. He thence concludes, that every perpendicular object, of whatsoever matter composed, possesses at its upper end a magnetic south pole, and at its under end a magnetic north pole. This result is of such importance, that we hope some of our philosophers and astronomical observers, accustomed to minute precision of research, will verify these experiments of the learned Dane.

From a comparison of Col. Beaufoy's observations on the magnetic declination needle, M. Arago has shewn, that, since the month of April, 1819, the direction of the movements of the needle

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variations are unfavourable to such an annual change as the Norwegian Professor indicates.

* Baron Humboldt made a similar remark some years ago at Berlin; but unless the observations of the horizontal needle be combined with simultaneous observations of the dipping-needle, the variation of intensity cannot be held to be demonstrated.

has become retrograde; the mean annual amount of which is $= - 1' \, 57''$. The total retrocession between 1818 and 1820, by a comparison of the morning observations, is 3′ 22″
noon .... 4′ 22″
evening.............. 4′ 0″

The Board of Longitude of Paris has established lately, at the Observatory, a magnetic needle, exclusively consecrated to indicate the diurnal variations of declinations. Some derangement, from an unknown cause, having happened to it in 1819, it was re-mounted last February, since which time its march has been regular. The observations made with it indicate already a retrograde movement of the needle towards the east. The *mean declination* of the month of February, 1821, is smaller by 2′ 15″ than that of the month of February, 1820.—*Ann. de Chim. et de Phys.* xvi. p. 54.

We reserve several electro-magnetic notices for next quarter.—We intended to have given M. Moll's account of Colonel Offerhaus's apparatus, published in the *Journal de Physique* for April last, in the form of a letter to the editor; but, as we find that the same letter is re-printed, with the date of June 22d, in the *Edin. Phil. Journal* of October last, we are saved the trouble of transcription.

IV. Under the science of MECHANICS, the most prominent paper is that of M. P. S. Girard, on the uniform discharge of atmospherical air and carburetted hydrogen gas, through conducting pipes. We have no room for an analysis of it at present.

V. NATURAL HISTORY.—Baron Humboldt, in a memoir read to the Institute 19th February last, 1821, entitled, "New Observations on the Laws which we observe in the Distribution of Vegetable Forms," states, that we already know nearly 56,000 species of cryptogamous and phanerogamous *plants*, 44,000 insects, 2,500 fishes, 700 reptiles, 4,000 birds, and 500 species of mammiferæ. In Europe alone, according to the researches of M. Humboldt and M. Valenciennes, there exist nearly 80 mammiferæ, 400 birds, and 30 reptiles. There are, of consequence, under this temperate boreal zone, 5 times as many species of birds as of mammiferæ; as, in like manner, there are in Europe 5 times as many compositæ as amentaceous and coniferous plants; 5 times as many leguminous as there are of orchideous and euphorbiaceous. The fine collections recently brought home from the Cape of Good Hope by M. Delalande prove, (if we compare them with the works of M. M. Temmink and Levaillant,) that in that part of the temperate austral zone, the mammiferæ are also to the birds in the proportion of 1 to 4.3. Such an accordance between two opposite zones is very striking. The birds, and especially the reptiles, increase much more towards the equatorial zone than the mammiferæ. According to the discoveries of M. Cuvier on fossil bones, we might believe, that these proportions have not been the same at all times; and that there have disappeared, in the ancient catastrophes of our planet, many more mammiferæ than birds. We can conceive how, on a given space of territory, the individuals belonging to different tribes of plants and animals may be *numerically limited;* how, after an obstinate struggle and long oscillations, a state of equilibrium comes to be established, resulting from the necessities of nourishment and the habits of life: but the causes which have *limited the forms* are hid under an impenetrable veil, which withdraws from our view whatever relates to the origin of things, or to the first developement of organic life.

On the preponderance of certain families of plants depends the character of the landscape; the aspect of a smiling or majestic nature. The abundance of gramineæ which form vast savannahs, that of palms and coniferæ, have had a powerful influence on the social conditon of nations, on their manners, and the more or less rapid developement of the useful arts. Sometimes a single species of plants, especially among those styled, by M. Humboldt, *social*, covers a vast extent of country. Such are, in the north, the heaths, and forests of pines; in equinoctial America, the union of cactus, croton, bambusa, and brathys of the same species.—*The sequel of this will be given in our next Number.*

GEOLOGY.—Mr. Fox having communicated to the editors of the *Annales de Chimie et de Physique*, in a manuscript letter, new determinations on the temperature of the earth at various depths, these gentlemen have published them, along with an extract made by M. Fourier, from his profound geometrical researches on heat.

The observations were made in ten different mines; *viz.*,—Dolcoath, United Mines, Treskerby, Whealsquire, Ting-Tang, Wheal-Gorland, and Wheal-Damsel (copper-mines); Chase-Water (mine of tin and copper); Wheal-Unity (in the tin part of the mine), and Wheal-Vor (tin-mine). The following is a list of the temperatures observed. The numbers inscribed on the table are the means of particular results obtained for each depth in the *ten* mines above-named.

At the depth of 10 fathoms......	Temp. 50.18° F.—	10.1 C.
20 to 30...............	60.98	16.1
30 to 40...............	59.54	15.3
50 to 60...............	62.06	16.7
60 to 70...............	63.86	17.7
70 to 80...............	65.84	18.8

At the depth of 80 to 90	68.36° F.	—20.2 C.
90 to 100	69.80	21.0
100 to 110	68.54	20.3
110 to 120	69.98	21.1
120 to 130	69.62	20.9
130 to 140	72.14	22.3
150 to 160	75.02	23.9
190 to 200	75.92	24.4
at 230	78.44	25.8
at 240	82.04	27.8

In a great number of cases, the thermometer was sunk in the rock to the depth of 6 or 8 inches; sometimes the temperature of the air or water was merely taken; generally both methods afforded results slightly differing from each other. The thermometer, plunged in the metallic veins, usually indicated a temperature of 1° to 2.8° centigrades (1.8° to 5° F.) higher than that obtained when the thermometer was plunged into the hole of a rock, and particularly in granite. The veins of tin are commonly a little colder than the veins of copper. In the bottom of the mine of Dolcoath, at 240 fathoms of depth, there issues from the vein a very abundant jet of water, whose *constant* temperature is 27.8° C. (82.04° F.) What more evident proof can be given, says Mr. Fox, of the great heat of the interior strata of the globe? The draining-pumps of the *United Mines* were not long ago totally deranged by an accident; two galleries, one at 200, and the other at 190 fathoms in depth, were found eventually filled with water. This state of things lasted two entire days. As soon as the water was pumped out, and before the workmen descended to resume their labours, Mr. Fox ascertained the temperature of the two galleries; that of the first was 31.1° C. (87.98° F.) The thermometer in the second, at a depth of 190 fathoms, continued stationary at 30.8° C. (87.44° F.) It ought to be remarked that, in order to avoid all error in the observation of these temperatures, the thermometer had its bulb plunged several inches under the floor of the galleries. Mr. Fox analyzed the waters of *Dolcoath*, and those of the *United Mines*, whose temperatures are so high, and he found in the first only a small quantity of muriate of lime, and in the others a proportion equally small of the sulphates of iron and of lime. Mr. Fox adds, that new experiments made in coal-mines confirm perfectly the results obtained in the galleries of the mines of copper and tin. Thus in a colliery,

At 10 fathoms depth, the temperature was	50° F.	10° C.
36 . . .	57.92 —	14.4 —
90 . . .	62.06 —	16.7 —

Of M. Fourier's speculations on the above curious facts, we shall give some account in the next Number.

The following extract from a letter of Count Mercate, describing the earthquake in the Isle of Zante, 29 Dec. 1820, may perhaps be found interesting among geological phenomena: "Towards midnight," says he, "I heard a hollow and interrupted noise, which appeared to issue from the bosom of the earth. This noise resembled the sound of a drum, beat from time to time in a subterraneous vault. It was heard by the greater number of persons who were awake at the time. We passed the night in a state of horror, and at ten minutes before four o'clock in the morning a sudden blast of wind, of an extraordinary violence, made us imagine the end of the world had arrived. This wind instantly subsided into a dead calm. Presaging the impending misfortune, I experienced an inward indescribable horror. In this melancholy mood I had thrown myself into bed, when I felt struck all at once by a horrible subterranean bellowing, announcing the commotion of the earth, which immediately ensued. I instantly rose up, but the violence of the shocks made me fall back on the bed. These concussions were threefold; the first, of great violence, was vertical; the second produced an undulatory movement; and the third, which was the most powerful, gave a rotatory motion. The most solidly-built houses could not resist the violence of these earthquakes. Eighty were entirely overturned, nearly eight hundred were horribly shattered, and the others so damaged as to be uninhabitable without being repaired. A confused and sudden noise of shouts and crying announced the universal alarm of the population, who thought their last hour was come. But in the midst of so many disasters, it is remarkable that only four persons were killed, and a few others wounded by the ruins." He speaks in very warm terms of the liberal charity of Lord Strangford, ambassador to the Porte, who was then in the harbour, as well as of the Lord High Commissioner, Sir P. Ross.

Three or four minutes before the first shock there was seen, at the distance of two leagues from the promontory of Geraca, to the south-east of the island, a fiery meteor, almost swimming on the sea, which remained lighted for five or six minutes. The following day there was a meteor, which blazed forth at four o'clock in the evening, and describing in the air a vast parabola from east to west, fell into the sea.

METEOROLOGY. See *Meteorolites*, under ANALYSIS.—M. Vogel of Munich, while spending some days on the banks of the Baltic, was told that different invalids, labouring under asthmas, &c., found themselves much better on sea than on shore. He then examined the air in different situations, and found that the sea air a league from shore, when admitted into an exhausted globe containing barytes water, did not trouble it, whereas the air, admitted into a similar globe on the shore, produced immediate turbidity. Nitrate of silver in solution, exposed to sea air, had some chloride formed in it. Hence he infers that the atmosphere of the Baltic, taken at a league from the shore, contains less carbonic acid than the ordinary atmosphere, and that it is probable the quantity of carbonic acid diminishes as we recede from the land. 2d, That the atmosphere of the Baltic contains muriates in greater or less quantity *.

VI. ECONOMICS.—In pulling down lately the vestry wall of a chapel, near the Lago Maggiore, which had been built more than 300 years ago, as appears by good documents, there was discovered, imbedded in the mortar of the wall, three eggs, which were found to be fresh. M. Cadet, one of the editors of the *Journal de Pharmacie*, after relating this fact, states that naturalists bring from America and India birds'-eggs, covered with a film of wax, which, after removing the wax with alcohol, may be hatched. He then talks of a man who sold eggs at the public market in Paris, which had been preserved upwards of a year in a peculiar composition. A slight layer of carbonate of lime observed on these eggs induced M. Cadet to suspect that lime-water was the preservative composition. He afterwards made experiments on this point, under direction of the Council of Salubrity of Paris, and succeeded in keeping eggs perfectly sound during nine months and ten days, the period of the experiments. We believe this means of preserving eggs has been long known to housekeepers in this country, but it is less practised than it deserves to be. If every farmer would cause the eggs of his poultry to be put into a cask of lime-water the moment they were laid, the inhabitants of London might enjoy better breakfasts than they do at present.

Uninflammable Clothes.—M. Gay-Lussac announced in the sitting of the Academy of Sciences, 6th Nov. 1820, that linen dipped in a solution of phosphate of ammonia became incombustible. MM. Merat-Guillot, father and son, apothecaries at Auxerres, have since shewn that the acidulous phosphate of lime possesses the same property. In fact, linen, muslin, wood, paper, straw, impregnated with a solution of this salt at 30° or 35° of concentration (1.26 to 1.30), and dried, became absolutely uninflammable, and consequently unfit to communicate fire. They carbonize, or char, when they are exposed to a very intense flame, but the carbonization does not extend beyond the focus of heat in which they are plunged.

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* *Journ. de Pharm.* Oct. 1821, p. 461.

ART. XIV. ANALYSIS OF SCIENTIFIC BOOKS.

i. *Fumifugium; or, the Inconvenience of the Aer and Smoake of London dissipated. Together with some Remedies, humbly proposed, by* JOHN EVELYN, Esq., *to his sacred Majestie, and to the Parliament now assembled. London,* 1661.

As we are not restricted to the notice of modern scientific books, we have selected the above scarce tract for the purpose of comparing the grievances occasioned by the smoake of London 160 years ago, when the metropolis was not one-sixth its present extent, with those which are now matter of complaint; and of inquiring how far the evil was then, and is now, susceptible of diminution, or removal.

It is curious enough that Mr. Evelyn's attention was called to the subject before us by "a presumptuous smoke issuing from one or two tunnels neer Northumberland-house, and not far from Scotland-yard," the very seat, if we are not misinformed, of the plots of our modern fumifugists; he therefore prepared the "short discourse" before us, for the reformation of this nuisance, and in the hope of rendering "London one of the sweetest and most delicious habitations in the world, and this with little or no expense;" being amazed, he says, "that where there is so great an affluence of all things which may render the people of this vast city the most happy upon earth, the sordid and accursed avarice of some few particular persons should be suffered to prejudice the health and felicity of so many."

It is also not a little remarkable, that although Mr. Evelyn's *Fumifugium* was written before the great fire of London, many of the evils and nuisances, to which he adverts, are still extant, and in full force: a few of them, though perhaps not the most pressing, either have been remedied, or are likely to be amended. This glorious and ancient city still wraps her stately head "in clowds of smoake and sulphur, full of stink and darknesse," most of our streets are still "narrow and incommodious in the very centre, and busiest places of intercourse," and we still have to deplore the "frequent wharfes and magazines of wood, coal, boards, and other coarse materials, most of them imploying the places of the noblest aspect for the situation of palaces towards the goodly river:" at the same time we need now no longer complain, "of our streets being composed of a congestion of misshapen and extravagant houses; of the ill and uneasy form of the paving under foot," nor of "the troublesome and malicious disposure of the spouts and gutters overhead."

After adverting in the first part of this essay to the transcendent situation of London, "built upon a sweet and most agreable eminency of ground at the north side of a goodly and well-conditioned river, towards which it hath an aspect by a gentle and easie declivity;" to the advantage of the gravelly soil, and to the plentiful and rich supply of water, every where good and abundant, our author proceeds to point out the origin and effects of that "hellish and dismall cloud of sea coale, which is not only perpetually imminent over her head *(conditur in tenebris altum caligine cœlum)*, but so universally mixed with the otherwise wholesome and excellent air, that her inhabitants breathe nothing but an impure and thick mist, accompanied with a fuliginous and filthy vapour, which renders them obnoxious to a thousand inconveniences, corrupting their lungs, and disordering the entire habit of their bodies." This is a strong but not exaggerated picture of the state of this monstrous grievance in the year 1661; and, when we reflect upon the gradual increase of the evil up to the present period, we cannot but hail the recent enactments of the legislature directed towards the consumption of smoke, wishing that the subject may receive that serious attention which it so urgently requires. We regret, however, that experience justifies a doubt as to the efficacy of mere parliamentary interference in a matter of this kind; whilst novelty recommends it, and the reports of Committees serve as a vehicle for the publication of individual schemes, all goes on with eagerness and energy; but as soon as parliament is prorogued, and the smoke-burners out of town, we relapse into our pristine fuliginosity, and the pretty-behaved chimneys upon the river-side, which awhile seemed to have forgotten their office, again evolve their wonted columns of sable smoke, "belching it forth," as Evelyn says, "from their sooty jaws, and making the city of London more like the suburbs of hell, than an assembly of rational creatures: so that the traveller, at many miles' distance, sooner smells than sees the place to which he repairs." We might quote, as a parallel instance, the Bill for the removal of nuisances from the streets, which for a time was sufficiently effective, and contributed greatly to the comfort of the metropolitan pedestrian; lately, however, the beggars have resumed their ancient stations, the pavement is as heretofore decorated with the praises of our radical patriots, written, as they should be, "all upside down;" barrow-women vociferate the contents of their moveable magazines in the most audacious defiance of street-keepers, constables, and acts of parliament; hackney-coachmen quit their boxes, and carmen mount their drays with unreserved impunity; the White Horse cellar continues, as heretofore, the eternal and undisturbed resort of stage-coachmen, jews, and pickpockets; Carlile's shop remains open for the edification and instruction of the young of both sexes; stalls and other peccant excrescencies are again shooting up at street corners; and, in short, every thing shews, that unless the orders of the legislature are enforced by a respectable and well-directed police, not active by fits and starts, but permanent and regular in its operations, all parliamentary interference in these matters is utterly ineffectual, and even mischievous, whenever laws are infringed with impunity.

It appears, to return to our subject, that in Mr. Evelyn's time, brewers, dyers, lime-burners, and salt and soap-boilers, were the principal nuisances; and since then, says the editor of the new edition of the *Fumifugium*, published in 1772, "we have a great increase of glass-houses, founderies, and sugar-bakers, to add to the black catalogue, at the head of which must be placed the fire-engines of the water-works at London-bridge and York-buildings, which leave the astonished spectator at a loss to determine whether they do not tend to poison and destroy more of the inhabitants by their smoke and stench, than they supply with water." To this sooty list, what astonishing additions have been made within the last thirty years, in and about London—how many new water companies, and smoke-producing manufactories have been added to the catalogue. A newspaper cannot now be printed, nor a pound of meat minced for sausages, without a steam-engine: to the same smoky servant the druggist resorts to grind his rhubarb, and sift magnesia; and upon all possible occasions the service of the other elements is superseded by that of fire. The natural consequence is, that the smoke of London, always grievous, is now scarcely tolerable: to select a few of Mr. Evelyn's *miseries*, "it obscures our churches, makes our palaces look old, fouls our clothes, and corrupts the waters, so that even the rain and dew are contaminated. It is this which scatters and strews about those black and smutty atoms upon all things where it comes, insinuating itself into our most secret cabinets and precious repositories; yea, though a chamber be never so closely locked up, men find, at their return, all things that are in it covered with a black soot, and all the furniture as full of it as if it were in the house of some miller, or a baker's shop, where the flour gets into their cupboards and boxes, though never so close and accurately shut." "Finally, it is this which diffuses and spreads a yellownesse upon our choicest pictures and hangings; which does mischief at home, is *avernus* to fowl, and kills our bees and flowers abroad, suffering nothing in our gardens to bud, display themselves, or ripen." "Not therefore to be forgotten," continues our author, after some further grumblings, "is that which was by many observed; that in the year 1644, when Newcastle was besieged and blocked up in our late wars, so as through the great dearth and scarcity of coals these fumous works were either left off or diminished, divers gardens and orchards, planted even in the very heart of London (as, in particular, my Lord Marquis of Hertford's, in the Strand; my Lord Bridgewater's, and some others about Barbican), were observed to beare such plentiful and infinite quantities of fruits, as they never produced the like either before or since, to their great astonishment; but it was, by the owners, rightly imputed to the penury of coales, and the little smoke which they took notice to infest them that yeare."

It would be to no purpose further to accompany our author through this region of practical grievances; we shall, therefore, pass on to that part of the tract relating more especially to the influence of London smoke upon the health of its inhabitants. It has sometimes been argued that the fuliginous atmosphere of London is rather salutary than otherwise, and that, at all events, it tends to counteract contagion, and disinfect the air; it has also been contended, that the mortality of its inhabitants has not sustained an increase proportionate to their augmented numbers. We believe it must be admitted, that the town has not become less healthy within the last fifty years, but we doubt whether it is more so, which, considering the enormous supplies of water which inundate every district, the widening of several streets, the removal of many of those nests of filth and poverty which disgraced some of the more prominent parts of London, and, above all, the general amelioration of the sewers and drainage, ought indisputably to have been the case. There are so many contingent circumstances that interfere with our estimate of the mortality of the inhabitants of large cities, that it is very difficult to form a just conclusion as to the effects of individual causes; yet, viewing, the subject in this light, we cannot, however willing, ascribe any serious mischief to the mere smoke; let us, for instance, look at the mortality of Paris, which equals, or even exceeds, that of London, yet its inhabitants enjoy a most sweet and delightful air; the sky is clear and serene; the foggy obfuscations and jaundiced composts of soot and vapour which adumbrate London are there unknown, and our vision extends nearly at all times and seasons from one end of that splendid metropolis to the other; whereas here we consider it wondrous clear when we can discern the Monument from Blackfriars-bridge, or see St. Paul's Cross from the bottom of Ludgate-hill. If indeed we adopt the maxim, *non est vivere, sed valere, vita*, we must then concede all to the inhabitants of Paris; for who has not felt the vivifying influence of their undisturbed and transparent atmosphere, in which every object has a sharp and cleanly outline, and figured to himself the magnificence of London, were it blessed with such a climate. To the circumstance of breathing an air thus pure and uncontaminated, some of the peculiarities of the English at Paris may possibly be referred; and the celebrity of the French *cuisine* among our countrymen is not perhaps altogether independent of such a cause. "I report myself to all those, " says Evelyn, " who have been compelled to breathe the air of other countries for some years, if they do not now perceive a manifest alteration in their *appetite* and clearnesse of their spirits, especially such as have lived long in France and the city of Paris." But Evelyn attributes more to the smoke than can well be substantiated, and consequently often ascribes effects to its absence, which are referable to other causes. "Although," says he, "London is tolerably free from the plague, it is never clear of smoke, which is a plague so many other ways, and indeed intolerable, because it kills not at once but always, since still to languish is even worse than death itself. For is there, under heaven, such coughing and snuffling to be heard as in the London churches and assemblies of people, where the barking and spitting is incessant and most importunate. What shall I say ? *hinc hominum pecudumque lues*." He then goes on to inform us, that the cause of these effects is the inhalation of this "infernal vapour," which irritates the windpipe, together with those multiform and curious muscles, the immediate and proper instruments of the voice, which becoming rough and dry, can neither be contracted nor dilated for its due modulation, so as by some of my friends studious in music, it has been constantly observed that, coming out of the country into London, they lost three whole notes in the compass of their voice, which they never again recovered till their retreat."

If therefore we consider all these evils, and "what a fuliginous crust is yearly contracted and adheres to the sides of our ordinary chimnies, and then imagine if there were a canopy over London what a mass of soot would stick to it which now comes down into the streets, houses, and waters, and is taken into our bodies," it is certainly somewhat surprising that the means of prevention have not been more attentively studied; what these are, and to what extent the proposals for the diminution of smoke and of the various nuisances dependant upon it, have been, or are likely to be carried, we may now proceed to examine.

Mr. Evelyn's plan, which once might have been feasible, is at present out of the question. It consisted in the removal of all nuisance-involving trades from London "five or six miles down the river Thames, or at the least so far as to stand behind that promontory, jutting out and securing Greenwich from the pestilent air of Plumstead Marshes." This is all he has to offer; he then proposes gardens and plantations in and about the metropolis, and enumerates a variety of fragrant plants suited to our climate, and calculated to sweeten and improve the air. It appears probable that the lime trees in St. James's park were planted in consequence of this suggestion.

Among contingent metropolitan nuisances noticed by Mr. Evelyn there are two, which to the disgrace of the present age and in spite of our boasted amelioration and refinement, still exist in full force, namely, burying-grounds and charnel-houses around and under churches, and the "horrid stinks" of our markets; the former, as our author justly observes, independent of the disgusting spectacle of skulls and skeletons which they continually exhibit to the passenger's notice, must tend to contaminate the air; and not the air only, but the water also, for there is a strange custom of tacking a pump to our church-yard walls, the well of which is generally surmounted by heaps of corpses in various states of putrefaction and decay, piled up and kept together by a kind of terrace-wall, which the enormous accumulation of human remains renders a necessary appendage to most of these teeming receptacles of mortality. In respect to our markets, Evelyn's complaints also hold good; the filth of our slaughter-houses and the abominations of Covent-garden, our *marché aux fleurs*, are familiar to every one; but these are matters not now immediately before us, however deserving of that aid which the resources of science have elsewhere lent them; the cemeteries and markets of Paris might especially be resorted to as containing the elements of admirable arrangements.

There is another growing nuisance, to which the following passage from the *Fumifugium* is not inappropriately applicable: we mean the *gas-works*, which have already destroyed the smelts and flounders in the immediate vicinity of London, while the Brentford establishment threatens a similar annihilation of the finny tribes of Isleworth and Richmond, and even promises to interfere with the honest angler's sport as high up the river as Ham and Twickenham: "If," says Evelyn, "we may not hope for so absolute a cure of all that is offensive, at least let such whose works are on the margent of the Thames, and which are, indeed, the most intolerable, be banished further off, and not once dare to approach that silver channel, which glides by her stately palaces, and irrigates her welcome banks."

But it is now time to acquaint our readers with the recent plans and proposals for the destruction of smoke, premising, however, that there is nothing new in them, for they have been canvassed and considered with various ability and success, by almost all our first-rate engineers, at the head of whom we place the late celebrated Mr. Watt, who long ago turned his mind to this subject, and in our opinion, achieved much that has erroneously been given to his contemporaries and successors; indeed, the great engines at the Soho manufactory, have all along been worked without smoke, and we are a little surprised that in the Report, now lying before us, from the committee appointed by the House of Commons, "To inquire how far it may be practicable to compel persons using steam-engines and furnaces in their different works, to erect them in a manner less prejudicial to public health and public comfort," and upon which report the bill of last session was founded, that no notice is taken of Mr.

Watt's suggestions and inquiries. To establish his priority of invention, we shall quote a few lines from the specification of his patent as printed in the *Repertory of Arts* for 1796*. "My newly improved methods of constructing furnaces or fire places, consist in causing the smoke or flame of fresh fuel in its way to the flues or chimneys, to pass, *together with a current of fresh air* through, over, or among fuel which has already ceased to smoke, or which is converted into coke, charcoal, or cinders, and which is intensely hot; by which means the smoke and grosser parts of the flame by coming into close contact with, or by being brought near unto the said intensely hot fuel, and by being *mixed with the current of fresh or unburnt air*, are consumed, or converted into heat, or into pure flame, free from smoke." Mr. Watt's specification then goes on to describe the construction of the fire-place and flues, and continues thus: "My said invention consists only in the method of consuming the smoke and increasing the heat, by causing the smoke and flame of the fresh fuel to pass through very hot funnels, or pipes, or among, through, or near fuel which is intensely hot, and which has ceased to smoke, and by *mixing it with fresh air, when in these circumstances.*"

We do not, however, mean to claim for Mr. Watt the original idea of burning smoke, by causing it to pass through hot fuel, but merely to shew his merit in applying a smoke-consuming apparatus to furnaces of engines, and other fire-places producing large quantities of black and inconvenient vapours. Franklin, as our readers probably know, was a great chimney-doctor, and suggested a similar means of getting rid of smoke in 1785†. But, long before Franklin, namely, in 1682, Mr. Justell read to the Royal Society, "An account of an engine that consumes smoke, shewn lately at St. Germains' fair in Paris‡." This "engine," as it is here called, was merely a chaffing-dish with a descending flue, so that the fumes and smoke of substances burned upon it passed downwards through the ignited fuel and were in that way destroyed. In 1723, too, a Dr. Leutmann of Wirtemburgh, described in his *Vulcanus Famulans*, "a stove which draws downwards." We thrown out these notices merely to shew the little originality of the contrivances of the Marquis de Chabannes, and others who have burned their smoke by a downward draught of air.

In the Parliamentary Report above alluded to, there are two inventions for the destruction of smoke, which appear principally to have occupied the attention of the committee, and which also profess to accomplish that desirable object with a very considerable saving of fuel.

Mr. Brunton is the patentee of one of these inventions; he applies to the engine-boiler a newly-constructed fire-place, containing a circular grate, which is made slowly to revolve upon its axis; the fire upon this grate is fed in front by a kind of hopper, continually delivering small coal, which, from the rotary motion of the grate itself, becomes equally spread upon its surface, so as to maintain a thin fire and a sharp draught; the coal is thus rapidly and perfectly decomposed and burned, the smoke at first produced having to pass across the grate and over the red-hot and already coaked fuel. The great advantages of this plan consist in the uniformity of heat, and its proportionate production to the work which the engine has to perform, or to the quantity of steam consumed; the fire-bricks are not injured; the clinkers or scoriæ are produced in thin layers; and the bars are so little heated, that while three bushels of coal per hour are consuming, they are seldom hot enough to singe paper; the boilers are less injured than by a common fire; and there is a considerable saving in the consumption of fuel. The disadvantages of Mr. Brunton's plan are, the expense of the apparatus; the requisite alteration of the boiler, or rather the addition of a supplementary boiler; the necessity of a moving power to effect the rotation of the grate; and the labour of breaking the coals into small pieces before they are put into the hopper. Upon the last point Mr. Brunton, when questioned by the Committee, replies as follows: "No coals should be put on a steam-engine fire until they are small enough to pass a three-inch mesh; therefore, the necessity of breaking the coal to that size is advantageous; but we have lately burned a species of small coal in our own furnace, and also in the town of Birmingham, which has till now been regarded as perfectly useless, and as such there are thousands of tons encumbering the ground in the Staffordshire collieries, incapable of being used with effect in any other furnace, and we have produced with this hitherto supposed rubbish, 70 per cent. of the effect of saleable coal *."

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* Vol. IV. p. 226. † *Memoirs of the Life and Writings of Benjami Franklin*, vol. vi. p. 408. ‡ *Phil. Trans.*

How far Mr. Brunton's invention bears upon the main point of our inquiry, namely, the consumption of smoke, will appear from the following evidence: Mr. James Scott Smith, of the Whitechapel Distillery, says, "We can consume the smoke to a very great extent, and although it is not completely invisible, yet it is never offensive; we never have any of those dark volumes of smoke which are the cause of so much complaint." Mr. Brancker, of Liverpool (a sugar-refiner), also gives evidence as to the great diminution of smoke effected by Mr. Brunton's "fire-regulator;" and both these gentlemen speak in terms the most unequivocal, respecting the saving of fuel,

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* *Minutes of Evidence*, p. 12.

amounting, according to their joint evidence, to from 30 to 38 *per cent.* Upon this subject, however, we greatly apprehend that such evidence is open to unavoidable fallacy. When any thing new is going on in a manufactory, the heads of the concern are generally themselves on the alert, and more than ordinary care and attention are bestowed upon all its details; we are pretty well convinced, and it is indeed obvious, that in any great establishment a monstrous saving of all materials, but of coals more especially, would be effected by the personal superintendence of the master, and of scientific persons well versed in the minutiæ of the concern; yet we are willing to allow, that one great merit of Mr. Brunton's contrivance consists in its being almost independent of the stokers and labourers; there is none of that eternal pitching of coals into the furnace which goes on with such lavish waste in all ordinary engine fires; "the grand principle in this machine," says Mr. Smith, "is, that it makes all stokers alike good, and they always use the same quantity of coals when doing the same work." This is, indeed, a great point carried. We think it but justice to Mr. Brunton to add, that private information derived from various quarters, confirms, to a considerable extent, the extracts we have made from the Minutes of Evidence; we are inclined to consider the saving of fuel quite as important as the consumption of smoke, and in that respect his pretensions seem indisputable; there is also much original ingenuity in his contrivance; indeed, we are not aware that a rotatory grate was ever before either devised or constructed.

Another very effectual, and in some respects, preferable method of consuming smoke, is the invention of Messrs. Parkes, of Warwick. These gentlemen, who are the proprietors of an extensive worsted manufactory, were greatly annoyed by the smoke of their engine-boilers, especially in their bleaching and drying ground; they have now so far effected its consumption that, for about twelve hours of the day, the smoke is nearly invisible, and there is no soot; moreover, these desirable objects are accompanied, they say, by a considerable saving in the article of coals.

In the plan adopted by Messrs. Parkes the boilers remain *in statu quo*; the fire-place is somewhat altered in shape and dimensions, but the principal agent, as far as regards the destruction of the smoke, is a current of air which is admitted just beyond the end of the fire-place, by means of an aperture which may be increased or closed at pleasure, and which they call an *air-valve.* A small fire is first made to burn brightly at the back of the grate; coals are then filled in towards the front, in which direction the fire gradually spreads; their smoke necessarily passes over the clear fire, where it becomes sufficiently heated to constitute flame, as soon as it meets with the current of air entering at the valve; and a striking experiment with this apparatus consists in alternately shutting and opening the air-valve, which is accompanied by the alternate appearance and disappearance of the smoke.

Even from this brief and incomplete view of Messrs. Parkes's contrivance, it is obviously preferable, in some respects, to that of Mr. Brunton: it is much less complex and expensive, and, when properly and assiduously attended to, it effects a more complete destruction of smoke; but, on the other hand, its success is infinitely more dependant upon the persons who manage the fire, and whose dispositions are generally a mixture of ignorance and prejudice, duly tempered with the warmth of the element over which they preside; we also doubt whether the same unequivocal testimony can be adduced in respect to the saving of fuel, for this again is more in the stoker's power. In point of originality, too, Mr. Parkes must undoubtedly yield the palm to Mr. Brunton; he is anticipated in every part of his invention by the words of Mr. Watt's patent; and we rather think that Mr. Gregson, of whose method of constructing chimneys an account will be found in a former Number of this Journal*, has also touched upon some of the most important parts of his contrivance. We, however, by no means state this opinion with a view to detract from Mr. Parkes's merit; on the contrary, if he has attained that in which Mr. Watt failed, the nearer his means approach to those unavailingly employed by his eminent predecessor, the more substantial is the service which he has rendered the public, and the more praiseworthy the perseverance by which he has accomplished his object. We believe that Mr. Watt never affected to combine diminution in the consumption of fuel with the destruction of smoke, and that, on the contrary, with additional expense and trouble, there were more coals burned; at least this is the only cause to which we can refer the non-consumption of smoke, and the non-application of his patent, in the greater number, if not in all, the large engines of his erection which we have had occasion to visit.

One subject seems to us to have been overlooked by the majority of smoke-reformers, namely, the construction, and more especially the height, of the furnace chimneys. By conveying black smoke, and other pernicious fumes into a capacious and very lofty chimney, much of the noxious matters that otherwise escape into the atmosphere are decomposed and precipitated, or condensed within; we were much struck some years ago with the effect of a long flue and lofty chimney attached to the steam-engine of the Dartford Powder-mills; scarcely any smoke issued from its funnel, the fuliginous particles being almost entirely deposited in the chambers of the flue. The chimneys of the Grand Junction engine at Paddington, and of the West Middlesex Water-works at Hammersmith, are more illustrative instances; when these machines are at work, the former produces little smoke; the latter inundates the neighbouring gardens with perpetual showers of the solid soot, and is the greatest of all conceivable nuisances; yet the only difference is in the height of the two chimneys, the boilers being in all respects set and constructed alike. Besides, if a high chimney does void smoke, it is generally wafted away and dissipated, except under particular circumstances of wind and weather.

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* Vol. III. p. 348.

As to the requisite height of a chimney for the diminution of nuisance from smoke, we are not prepared to give an opinion; it will depend very much on the circumstances of the case; from 150 to 200 feet would, we presume, in most instances, prove effectual; the expense of such a structure may certainly be urged against the proposal, but we are to recollect that one shaft might receive all the tributary fumes of many flues, and that a great saving would be effected in reducing the number of chimneys of medium height. The intolerable nuisance of brewers' chimneys, to whose coppers we fear neither Parkes's nor Brunton's inventions are applicable, would in this way be greatly diminished, if not altogether remedied.

Among other probable causes of the diminution of smoke, we look to the employment of steam as a substitute for fires; in Whitbread's brewery high pressure steam is thus very extensively employed, and, although they make quite smoke enough, it certainly has lately sustained a very perceptible diminution.

Lastly, we come to the most absurd portion of the speculations of theoretical smoke-burners, namely, the improvement of the atmosphere of the metropolis. Though some are sceptical upon the subject, we will admit that, if no smoke were made in London for a twelvemonth, or if wood-fuel were substituted for coals, there might be some amelioration of the atmosphere, although our locality and climate always render it turbid and misty, independent of adventitious effluvia; it is, however, folly to ascribe any sensible influence upon the great mass of London smoke to some few steam-engine chimneys, while every house is busy in the work of contamination, and every street yielding a proportion of filth far exceeding that of any single furnace, though less observed, because administered by separate vents, and in divided doses

When the builders of ordinary houses shall find it worth while to turn their attention to the consumption of smoke, and to display a little more common sense, as well as science, in the mode of warming and ventilating our dwellings; when architects, instead of confining their studies to the dimensions of temples and the measurement of pyramids, shall condescend to consult and visit the chemist and the natural philosopher, and to become acquainted with the doctrines of heat and of pneumatics, then, and not till then, may we expect a diminution of the evils which form the subject of this article of our Journal; and although we are not sanguine enough to anticipate the re-appearance of orchards in the Strand, or vineyards in Barbican, we should then be justified in looking for a degree of relief from those nuisances and evils which smoke inflicts upon the inhabitants of London. C.

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ii. *One Thousand Experiments in Chemistry, with illustrations of Natural Phenomena, and practical Observations on the Manufacturing and Chemical Processes at present pursued in the successful cultivation of the useful Arts, with numerous Engravings on Wood and Copper.* By COLIN MACKENZIE. London, printed for Sir Richard Phillips & Co., 1821. Price 21*s.* in boards.

WHEN we consider the manifold and marvellous changes which chemistry effects on natural products, converting the dull and brittle stone into a brilliant metal, to whose adamantine temper the forests yield, and even rocks give way; turning the bland inflammable sulphur into a corrosive acid, which extinguishes flame; extracting from calcined animal bones a substance that takes fire with the heat of the hand, and burns with exceeding splendour; eliminating from the ashes of a wood-fire metallic globules, which kindle and consume by the contact of ice; or evolving from culinary salt, an aërial element, in which refractory metals burn, even without the aid of heat; we can readily imagine that, in rude and remote times, the metallurgist Vulcan might be worshipped as a demi-god; that, in a more advanced state of civilization, but amid literary and scientific darkness, the chemical adept might be thought the elect of heaven, as in Egypt; or the copartner of Satan, as in Gothic Europe; and finally, that in the present period of philosophical light, the genuine chemist should rank high in society. The wonders and uses, rewards and honours, which flow from chemistry, have, however, this unpleasant effect; they tempt multitudes of unhallowed and uninitiated pretenders to rush into the sanctuary of science; and, though they are usually soon and unceremoniously expelled, yet they contrive to persuade the ignorant *profane*, that they possess in their own right mighty secrets, which they have in fact stolen from the very altar of Hermes.

As the English are allowed to surpass all other nations in their funds of cash and credulity, and as the chemical wants and wishes of their manufacturers are numerous and important, it has happened that chemical empiricism has of late begun to rival the illustrious career of Brodum, Solomon, and Eady. One-half, at least, of our chemical factories are committed to the hands of quack-chemists, who, by offers of unlimited profits, dazzle the dark minds of their moneyed proprietors, and supplant men of sound views and principles, who disdain such base methods of delusion. What an instructive book might be made by narrating the senseless schemes and expenditure of our national fire and light companies, of various great printfields, dye-works, glass-houses, sugar-houses, vitriol, alum, and soda works, &c.

We believe ourselves warranted, from an extensive range of observation, in asserting, that a sum of money which would make some figure amid the interest of our national debt, is annually wasted by these crafty and improvident projectors who infest the chemical arts of this country. It has been too much the fashion of late to speak contemptuously of the arts and manufactures of France; but if our ingenious and active neighbours possessed our constitution and our coal-mines, their competition would soon leave the British manufacturer little to boast of, and would compel him to have recourse, more frequently than at present, to the lessons of real science. We beg leave to hold out the following facts, as a slight check on these narrow prejudices, which at once degrade and impoverish their possessors, by fostering notions of superiority on points where they are grossly defective. We also trust that this small *exposé* may induce our operative and consulting chemists to study with a little more industry the scientific principles of their art.

To the manufacturer of sulphuric acid the wholesale price of

	In London.	In Paris.
Sulphur is, per ton............	£ 7 7 0	£ 11 4 0
Nitre	27 0 0	76 0 0

and the wholesale price of sulphuric acid in London is 16*l.* 0*s.* 0*d.*
and.............................. in Paris....12*l.* 0*s.* 0*d.*

If we estimate, for the formation of that acid, 1 part of nitre to 9 of sulphur, though we know some French manufacturers employ only 1 to 10, we have the following proportions:

9×£ 7 7=£66 3*s.*		9×£11 4=£100 16*s.*
1× 27 0= 27 0		1× 76 0= 76 0
£93 3	{Cost of 10 tons of materials in each country.}	£176 16

Thus we see that, at the present prices, the materials in France cost about double what they cost in England; indeed fully so, if we consider the superior quality of our nitre compared with that employed in France; yet the product resulting from these materials is sold 25 *per cent.* cheaper in France than in England. Now, put our sulphuric acid manufacturers in the same predicament with the French, that is, suppose them to buy their materials as dear, and to sell their product as cheap, we know that every vitriol work would of necessity be shut. Even under the existing advantages many of them are making scarcely any profit.

The truth is, that the principles of the formation of oil of vitriol depend on the solution of a chemical problem of four conditions, or where four conflicting affinities are concerned, *viz.*, those between sulphurous acid, nitrous gas, oxygen, and water. Now these affinities are modified in a very essential manner by the quantity of nitrogen present in the mixture, and the degree of its temperature. Thus, a problem which requires a very clear chemical head to regulate, is generally left to a very rude operative hand, or intrusted to a speculative impostor, who knows nothing of chemistry but a few technical terms. The consequence is, that a great part of the materials of that manufacture are sent off by such *managers* into the air, in the form of sulphurous acid and nitrous gas, to the great annoyance of the neighbouring animals and vegetables, and the ruin, too often, of the proprietor. " Drink deep, or taste not," is an adage much more applicable to the Hermetic, than the Pierian spring.

The work, the title of which is prefixed to this article, and of which the perusal has suggested these preliminary remarks, is admirably adapted to the meridian of the great tribe of chemical dabblers. It will furnish them, at no expenditure of intellect, either in its author or themselves, with the ready means of juggling the public. Contemplated simply as a literary object, it is perhaps the strangest farrago which ever issued from our press. " There is *no* method in its madness." It is calculated to give the same idea of chemistry as we should have of Homer or Milton, if the whole of the lines of their divine poems were transposed and printed at random. Indeed, such dislocations as these *thousand experiments* exhibit, appear even greater than hazard could have produced. Chance would now and then have tossed together two similar subjects, as in a thousand rattles of the dice-box two similar throws might appear in succession.

The elaborate confusion of its parts might be accounted for, on the hypothesis that the whole affair is a sort of literary larceny; in which case it would become necessary to disfigure and derange the stolen articles, to prevent their being recognised by their respective owners. This would afford the simplest solution of the Chemical *Millenary*.

It should have been named, however, after a far more edifying and instructive compilation from Arabia, the *thousand and one* experiments, for we conceive the experiment of such a book upon the British public much more surprising than any chemical wonder which its pages display.

There is a quarto plate fronting the title-page, which appears to us the best representation of Pandemonium yet contrived. It leaves Fuseli's genius far behind. This picture professes to exhibit the "drawing the retorts at the great gas-light establishment, Brick-lane," but of their construction and arrangement it communicates no definite idea. We see only a parcel of naked red figures, "smirched with fire," plunging huge pitchforks into the flames, and a number of black pot-lids, hanging apparently from a great iron pipe, with the moon peeping through a grated window, shedding disastrous twilight on the infernal scene. The publishers, however, are well aware that a flaming frontispiece often helps off a very indifferent book.

The author, if this monstrous production can be traced to any individual parent, not only disclaims all pretensions to methodical arrangement, but speaks with sovereign contempt of all system. "Regarding the order of the work, he would say, that, notwithstanding the elaborate researches and ingenious speculations of many learned men in the several departments of chemical science, there are very few phenomena so perfectly developed as to admit of a systematic arrangement of the principles deduced from them; consequently, any attempt at forming a system, where the foundation and other parts of the superstructure are incomplete, would prove abortive. It is for this reason that attempts to systematize chemistry have in every instance been productive of failure*."

We congratulate Mr. Colin Mackenzie on his perfect emancipation from the trammels of order, and we trust that the fate of his disorderly experiments will in future prevent raw hands from meddling with such dangerous articles as acids, alkalis, crucibles, and chlorine. But his modesty is as meritorious as his method. "The author, at the commencement, perceived that a strict adherence to the accounts and opinions *even of the most approved chemical writers* would be far from satisfactory to HIMSELF; and would, in many cases, prove delusive to his readers. He likewise foresaw that, although a general reader might, without further inquiry, acknowledge, or allow as true, every process and fact comprehended in the following multitudinous assemblage; some who should honour his labours by a perusal might be more fastidious; and, by calling in question the truth of a few particular facts, processes, or opinions, might be apt to pass sentence of condemnation upon the whole. These considerations, conjoined with an ardent desire, which he himself had, of becoming practically acquainted with several of the chemical arts, induced him, on many occasions, to seek for information at its ultimately genuine source, *viz.*, the workshop of the artisan †." To make this delusive profession still

* Preface, p. iv. † Idem.

more catching, the following motto is contrived for the title-page: "Give me the facts," said my Lord Judge, "thy conclusions are but the guess-work of imagination, which puzzle the brain, and tend not to solve this mystery."

We shall now shew, by a few quotations taken at hazard, how little commensurate is the certainty of *his* facts to his assurance, and how completely at variance are *his* details and those of practical chemistry, as they are to be found in the workshop of the intelligent artisan. The volume contains 528 pages, distributed into 20 chapters, and ornamented with 20 plates. The following is the order of his chapters: 1st, Metallic alloys, under which we meet with the sulphurets of iron and nickel, and the phosphurets of nickel and copper. 2d, Art of coating metals, &c., with metals, 3d, Separation of metals, &c., from their combinations. 4th, Expansion of bodies by heat, under which we have ebullition, sublimation, fusion, and finally, the blow-pipe of Dr. Clarke and Mr. Hare, spread over ten pages. 5th, Chemical affinity, of which the first experiment is, "Non-affinity of steel for water;" the second is, "solution unattended by change of bulk;" of this we shall talk anon. 6th, Elective affinity. 7th, Compound, or double, affinity. 8th, Experiments on crystallization. 9th, Experiments on the discovery of certain substances in combination with others, by means of tests. 10th, Preparation of gases. 11th, Experiments on respiration. 12th, Experiments on combustion. 13th, Preparation and uses of fulminating and detonating compounds. 14th, Of the evolution and absorption of heat. 15th, Experiments on the motion of caloric, and on the capacities of bodies for heat. 16th, Phosphorescence of mineral and animal substances. 17th, Specific gravity. 18th. Colouring and Bleaching. 19th, Fermentation and Distillation, 20th, Preservatives against animal and vegetable putrefaction.

The preceding list of the titles of his chapters will shew every person of the least discrimination into what confusion he has plunged the details of his work. But lest we be deemed of the number of "fastidious" persons, let us now see how far "his ardent desire of becoming practically acquainted with the arts" has carried him; and what kind of instruction he has acquired in his pretended visits to "its ultimately genuine source, the workshop of the artisan."

At *Exp.* 22, under *alloy of gold and platinum,* we find the following statement, entitled, "Observations." "It is a curious circumstance, that the alloy of gold and platinum is soluble in nitric acid, which does not act on either of the metals in a separate state." There is only one thing which lessens a little the curiosity of the circumstance, its being *untrue.*

Exp. 44. "*To make cast-steel.* Put 20 parts of pure iron in small pieces into a crucible, with 6 parts of powdered chalk, and 6 parts of powdered Hessian crucible-ware. Dispose the whole so that after fusion the iron may be completely covered, to prevent the least contact with the air; now give the crucible a gradual heat, and then expose it to a white heat. Generally an hour will be sufficient to convert two pounds of iron into exceedingly hard steel, capable of being forged; an advantage not possessed by steel in the usual manner. *Observations.*—Here the iron is formed into a carburet, by combination with the carbon of the chalk and crucible powder." This is the whole article. We should like to be told what English artisan communicated this process, or at what factory steel is thus made. It is Clouet's old scheme, which no Englishman acquainted with steel would practise.

" *Exp.* 114. *To obtain pure platinum.*" Here we have nothing but Ridolfi's preposterous process, which we trust no artisan in this country has ever practised.

" *Exp.* 120. *To prepare pure alumine.* To a solution of sulphate of alumine (alum) in water, add a solution of pure soda, as long as a white precipitate falls down. Here the sulphuric acid combines with the soda, leaving the alumine free. Dry the precipitate quickly, and preserve it in a phial." Now, as pure soda is an excellent solvent of alumine, the earth cannot be precipitated " free" in this way. Ammonia is the proper precipitant, and the precipitate may be dried as slowly as we please, after it has been thoroughly washed. Our author has here contrived, in the fewest possible lines, to display the greatest possible ignorance of *practical* chemistry.

" *Exp.* 171. *Solution not attended with change of bulk.* In the chemical combination of fluids with solids, the compound occupies no more room than the solvent did previous to combination. Put some sugar, muriate of soda (common salt,) or *any other salt!* into an ounce of water, until no more will be dissolved. The solution will measure just an ounce, as the water did before the addition of the salt; but although there is no increase of *bulk*, there is a considerable increase of density *and* specific gravity." One hardly knows how to treat such effrontery of falsehood, for every assertion here is contrary to the best established facts, and the plainest experiments. Water saturated with common salt has a specific gravity of 1.1962, and contains 25.5 per cent. of salt. The 74.5 parts of water, in dissolving these 25.5 of salt, whose specific gravity is 2.0, would, if there were no increase on the bulk of the solvent, acquire a specific gravity of 1.342. But the real increase of bulk on these 74.5 parts of the solvent is to 83.6 exactly. Were there not a condensation attending this exertion of chemical affinity, the bulk of the solution would be 87.212. Or, in round numbers, 30 parts by bulk or weight of water, dissolve

10 by weight of salt, and the solution occupies the bulk of $33\frac{2}{3}$ parts.

"*Exp.* 182. *The solution of bodies in acids intercepted by mechanical pressure.* Put into a Florence flask some powdered carbonate of lime (chalk), and pour over it some diluted sulphuric, muriatic, or nitric acid; immediate effervescence will be the consequence. Now stop the mouth of the flask with a cork; the effervescence will instantly stop, upon the same principle that fluids refuse to boil when the superincumbent pressure is, to a certain extent, greater than usual. If the cork be withdrawn, the effervescence will be resumed. *Observations.*—The agitated motion of bodies undergoing solution does not differ from ebullition; for in both cases the fluids vaporize; and when this pressure is used the ascension of vapour must stop, not having sufficient mechanical force to overcome the power of the body which presses upon it." We shudder at reading this experiment, as the Florence flask will certainly burst, to the imminent hazard of the operator's eyes. Indeed, the adviser of such a project should be made liable at law for the damages. It reminds us of the school-boy's attempt to extinguish a cracker by putting it into his breeches pocket.

"*Exp.* 183. *Solution of tin in nitrous acid.* Pour half an ounce of nitrous acid, over half an ounce of granulated tin (Pulvis Stanni) in a tumbler: very little action will take place, owing to the inability of both substances to present to each other a sufficient surface. But if an ounce of water be added, a very violent commotion will ensue, during which the tin, in an oxidated state, and of a yellow colour, will be seen to run up and down from the bottom to the surface, whilst a great quantity of nitrous gas is disengaged. This solution is the nitrite of tin." "*Observation.* The above solutions of tin, as will be seen hereafter, are much used by dyers." The dyer is to be pitied, who would try to make a solution of tin, in this way. In fact, the action above described ruins the process, and converts the tin into an insoluble peroxide, useless to the artisan. But this is not all: common nitrous acid invariably acts with great violence upon powdered tin, and a tyro might seriously injure himself by following Mr. Mackenzie's recipe.

Under *Exp.* 412, we find an encomium on the virtues of "Strathpeffer water, a very productive well, in Ross-shire, Scotland;" the district of the Clan Mackenzie. This internal evidence has partly satisfied us of the identity of the author whose name is on the title-page; we once thought it merely a *nom de guerre*.

At *Exp.* 469, p. 251, we find the following sapient remarks: "It is necessary to be observed here, that chlorine, and all other gases readily absorbable by water, should be received over *mercury*, in a trough made for the purpose of containing from a dozen to twenty pounds." So *practical a chemist* as Mr. Colin Mackenzie ought to know that mercury is the very substance employed to absorb chlorine in analytical researches on mixed gases; and that no *practical chemist* of common sense, ever tried to receive chlorine over mercury.

"*Exp.* 471. *Iodine vapour.* Put a small quantity of iodine into a retort, and hold it over a lamp; when heated considerably (about 300°) a very beautiful vapour, or gas, will come over, which may be received in jars on the pneumatic shelf, over water." Quere; is Mr. Mackenzie's water also heated to 300°; did he ever see, or hear, of iodine as a beautiful gas standing over water in the pneumatic trough?

"Combustion is the decomposition of a body at an elevated temperature, with the evolution of light and heat. Some suppose combustion to be the effect of a certain degree of motion of the particles of combustible bodies; and that flame is merely a transparency, or luminosity, of these particles when they are thrown to certain distances with considerable velocity*." One can hardly afford to waste criticism on such pompous nonsense. Combustion is known to be most vivid, when simple bodies are engaged; and when there is *no* decomposition, but the reverse, as with phosphorus and oxygen, sulphur and copper, *&c.*

"*Exp.* 529. *Combustibility of hydrozincic gas.*" This fine name merely signifies hydrogen procured by acting on zinc, as usual, with a dilute acid.

"*Exp.* 545. *Tin burns brilliantly in oxygen gas.* Heat some granulated tin considerably, in a platinum spoon, and in this state immerse it in oxygen gas; a very beautiful combustion, attended by a brilliant white light will instantly take place; when oxide of tin will be formed." The possessor of such a spoon had better not try this trick, for if the tin burns, which we have some doubts about, the spoon would melt, thus adding to the *instructiveness* of our author's experiment.

"*Exp.* 571. *A lighted taper burns with much energy in chlorine gas.*" It so happens that it is presently extinguished.

"*Exp.* 572. *Combustion of charcoal powder in chlorine.* Pour some dry charcoal, newly made and finely powdered, into a jar, containing chlorine gas; a very beautiful combustion will take place, displaying a stream of fire." Not true.

"*Exp.* 719. Latent heat is necessary to preserve bodies in the *solid*, liquid, and gaseous states." Dr. Black imagined that bodies would keep very well in the solid state, without the help of latent heat. Mr. Mackenzie has discovered that they will not.

"When bodies are mixed or combined, and the density or bulk becomes less than that of the fluids before mixture, heat will be evolved*" This absurd enunciation is for some unknown reason, printed in italics. He evidently does not know, that when the density of a body is increased by any means, its bulk becomes less, and *vice versâ!*

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* Page 281.

The chapter on specific gravity, is stuffed out with commonplace and ill-arranged details on the art of swimming, diving bells, weather-glasses, balloons, and air-pumps.

Chapter 18, entitled "*Colouring and Bleaching*," contains an unmerciful and unacknowledged plagiarism from a theoretical article on Dyeing, in the first supplement to the *Encyclopedia Britannica*. It can be of no service to the artisan.

"*Exp.* 867. *Bleaching by means of the Oxymuriatic gas.*" Towards the end of this article we have the following precious recipes. "The proportions observed when cotton is to be bleached, are manganese 30 parts, common salt 80, sulphuric acid 60, water 120. For linen cloth the proportions are as follow: manganese 60 parts, salt 60, sulphuric acid 50, water 50." We should be glad to know why 30 parts of manganese are sufficient for 80 of common salt in the first case, and 60 are required for 60 in the second. Nothing can place in a clearer light than these recipes, the irrational and dangerous empiricism of his pretended prescriptions for manufacturers. Again, "*Exp.* 870. To bleach by means of the sulphuret of lime." This old project of Mr. Higgins, was very unfortunate. If Mr. Mackenzie will take the trouble of inquiring of any intelligent Irish linen bleacher, (and there are many who understand chemistry infinitely better than he does,) he will find that the linen trade received such a shock by the employment of sulphuret of lime, instead of barilla, as it did not recover in reputation for several years. An immense deal of cloth was ruined by this notable recipe. Mr. Mackenzie's directions to bleachers remind us of those of an eminent agriculturist, (not less profoundly versed in *practical chemistry*), to farmers, who having heard of a plan for preventing the ravages of the fly in turnips, part of which consisted in steeping the seeds in a solution of chlorine: and having, moreover, heard that chlorine was produced by a mixture of oil of vitriol, salt, and manganese, committed the trifling chemical error of directing the seeds to be steeped in this mixture; a perfect cure, no doubt, for the fly, or any other evil to which the crop might have been liable.

"*Exp.* 997. *Copal Varnish.* Reduce to powder an ounce of pure carbonate of potash, and then lay it before the fire till it becomes hot and dry. In this state put it into a pint of alcohol, or oil of turpentine. All the watery particles contained in the oil, or spirits, will be absorbed by the alcali; and the alcohol, or turpentine, will thus become pure, or highly rectified. This process is called *alcalizing* these solvents. Now put the turpentine, or spirits, into a vessel with two ounces of clean dry copal, finely pounded and sifted; place the vessel in warm water; the copal will soon be dissolved." "Observations." "If the spirits of turpentine be alcalized when the copal is dissolving, a little spirits of wine should be added; and if the spirits of wine be alcalized when the copal is dissolving, a little spirits of turpentine should be added; the sediment of the varnish will dry on the silk in a few hours; the thicker the varnish, the sooner it dries." On this occasion, as with most other of his *practical* recipes, Mr. Mackenzie has been humbugged by the workmen to whom he has addressed himself. The above process is good for nothing. An ounce of dry carbonate of potash will not dephlegmate a pint of the alcohol of the shops, and the copal instead of being soon dissolved might remain there till dooms-day.

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* Page 349.

We presume, we have now satisfied every reader of ordinary intelligence, that this costly octavo is so devoid of scientific views, and so replete with practical errors, as to render its perusal useless to the student, illusory to the political economist, and deceitful to the artisan. Here and there, we meet with extracts from respectable authors, inserted often without acknowledgment, and always without direct reference to the source whence they are immediately derived. But, in general, the book is a *cento* of obsolete and exploded operations. We close these remarks, flowing solely and sincerely from a wish to protect our countrymen from deception and loss, with the following sentence, very slightly altered from the author's note to page 274. "It is hoped that none of the experiments will be repeated through wantonness." In this case, we are sure they will not be tried at all; for neither profit nor instruction can result from their repetition, as described by Mr. Colin Mackenzie.

iii. *Philosophical Transactions of the Royal Society, of London, for the year* 1821. *Part II.*

In resuming our account of the *Philosophical Transactions* for the year 1821, we shall lay before our readers a brief abstract of the principal facts contained in the several papers published by the Royal Society, examining them in the order in which they stand in the volume.

1. *An account of Experiments to determine the acceleration of the Pendulum in different Latitudes. By* Captain Edward Sabine, of the Royal Regiment of Artillery, F.R.S. & F.L.S.

To the merit of Captain Sabine, as an accurate and acute observer, we have more than once had occasion to advert; he is an exemplary instance of a military officer devoting his time and talents to the abstract pursuits of science, and voluntarily submitting to severe deprivations and personal danger, in the disinterested promotion of natural knowledge. We more particularly admire his various observations and inquiries made during the expeditions to the Arctic regions (under Captain Ross in 1818, and especially under Captain Parry in 1819 and 1820), for the skill and perseverance which they display in overcoming the untoward difficulties and unforeseen obstacles, arising out of situation and climate.

The clocks and pendulums, used by Captain Sabine in the researches described in this paper, belong to the Royal Society, and were prepared by their direction, under the superintendence of Captain Kater, whose description of them is quoted by the author at the commencement of his communication. The experiments were made during two voyages of discovery in search of a North-west passage, the first in 1818, and the second in 1819 and 1820; and Captain Sabine details in succession the proceedings at each station where an opportunity was afforded of landing and setting up the clocks, and concludes by recapitulating the number of vibrations made by each pendulum in the different latitudes in which it was tried, and by stating the deductions regarding the figure of the earth, which follow from the accelerations thus determined.

In the first voyage, the number of vibrations was ascertained at two stations only, namely, at Gardie-house, on the island of Brassa, and on Waygat or Hare Island, on the west coast of Greenland, the latitude of the first being 60° 09′ 42″ N., and that of the second 70° 26′ 17″ N. The number of vibrations in a mean solar day at London being 86497.4, at Brassa they were 86530.507, and at Hare Island 86562.6386, giving an acceleration of 33.107 vibrations between London and Brassa, and of 32.1316 between Brassa and Hare island, or 65.2386 between London and Hare Island.

Captain Sabine then proceeds to detail the preliminary experiments relating to the pendulums, and the results of his various observations, made during the second more auspicious voyage under Captain Parry, from which it appears that at Melville Island, in the Polar Sea, in lat. 74° 47′ 12.″4 N., the mean diurnal acceleration amounted to 74.734 vibrations. Of the proceedings at this station he gives a circumstantial account, which we from lay before our readers the following interesting details:

> As soon as the harbour was determined in which it was purposed to secure the ships for the winter, and whilst a canal was cutting to admit them through the ice by which it was already occupied, its shores were carefully examined, with a view to select a suitable spot for an observatory.
>
> The land was found of little elevation, and generally level, except where

intersected by ravines, being the courses in which the winter's fall of snow drained on dissolution to the sea. The soil, which appeared by the banks of these channels to be many feet in depth, consisted of sand intermixed with small stones, being the debris of the sandstone rock of which the island is composed; it was at this time consolidated by the frost, and was harder than the original rock, but much the greater part bore evident marks of being swampy at times; and even the more elevated spots afforded little prospect of a solid foundation for the clock-stands on the return of summer.

However, as no preferable situation could be found within such distance from the ships, as it would have been convenient, or indeed prudent, to venture, one of these was fixed on; and it was hoped that by sinking the legs of the stands a few inches into the frozen soil, and by commencing the experiments as early in the ensuing year as the season should admit, they might be completed before the ground should be affected by a thaw.

It was desirable therefore to be thoroughly prepared before the severity of the winter should set in; accordingly when the ships had been secured, and a party of men could be spared for the occasion, an observatory house was commenced. The house was built of the store plank and boards carried by the ships, care being taken to cut or injure them as little as possible: the walls were weather-boarded, lined, and filled in between with moss; the roof was protected by a tarpaulin covering; it was divided into two rooms, whereof the inner, being designed for the reception of the clocks, was warmed by pipes proceeding from a stove placed in the outer room; the floors were boarded, and the walls furnished on the inside with Russia matting. The house was finished and the clocks moved into it before the end of October.

If any hope had been entertained of being able to do more during the winter than merely to prepare for the return of more favourable weather, it was ended by the severity of cold, far exceeding expectation, with which November set in. From this date until the close of March, the highest degree registered by a thermometer, suspended in the air, was $+6°$ Fahrenheit, and in no one of these five months did the mean temperature rise above $-18°$; under such circumstances, an attempt to raise the temperature of the house, sufficiently to carry on the experiments, and to keep it up during their course, with the requisite steadiness and uniformity, must have altogether failed. It may not be amiss to remark, that notwithstanding the house was as effectual for the purpose as the utmost liberality in the supply of materials, with no labour spared in their application, could produce, a very little wind with so low a temperature abstracted the heat with such rapidity, that the influence of the stove was scarcely felt beyond its immediate vicinity; and a thermometer placed in those parts of the inner room where the clocks would have stood, could not be kept above zero, with such fire in the stove as it would have been prudent to maintain.

The clocks were therefore suffered to remain unpacked during the winter in the inner room, whilst the outer served a variety of useful purposes, which could not have been conveniently effected on board ship.

On the 24th of February, the matting with which the walls of the outer room were covered accidentally caught fire, and notwithstanding the endeavours of the persons who were present, the fire was communicated rapidly to the roof; it was at length fortunately extinguished by the exertions of the officers and men from the ships, before the clocks or any part of their apparatus had received injury; the packing chest alone of one was slightly scorched: the only personal sufferer on the occasion was an artilleryman, who had accompanied me on the voyage, and who, in his anxiety to place the instruments out of danger, exposed his hands incautiously, and was in consequence so severely frost-bitten, as to render necessary the amputation of three fingers of the left hand, and two of the right.

The house was speedily repaired, the outer room being reduced in size to a porch sufficient to contain the stove; and the inner room, which had scarcely been touched by the fire, remaining as before.

Towards the end of April the sun had influence to keep the thermometer a few degrees above zero for some hours of the day. The clocks were now unpacked and set up: the flooring being removed, the legs of the stands were placed on slippers sunk some inches into the frozen ground in grooves which were excavated by crow bars.

It may be worthy of remark, that when the boxes containing the thermometers which accompany the clocks were opened, the mercury was observed to be retired into the bulbs and freezing, although the temperature of the air had not been so low as the frozen point of mercury for several weeks. The thermometer boxes were enclosed each with the pendulum to to which it belonged, in a stout case of oak; and these again were contained in chests holding each one clock with its apparatus complete. The thermometers had been thoroughly cooled in their cases by the long continued severity of the winter; but the warmth had not yet made its way through such a multiplicity of enclosures. It may be also mentioned, in proof of the slowness with which such a mass of solid brass as constituted the bob of the pendulums conforms to the temperature of the surrounding atmosphere, compared with the mercury in the thermometer tubes, that several hours had elapsed, after the pendulums were taken out of their cases, (when it is presumed they also may have been at −40°) before they ceased to cause a deposit of moisture from the air of the room, which was about the same number of degrees above zero: the mercury in the thermometers, on the other hand, took up the temperature of the room within half an hour after their exposure. The clocks were put in motion on the 30th of April, and the account taken up on the 4th of May, the room having been kept at about the temperature of +45° for the preceding three days and nights.

It was not, however, till the third week in May that the weather became warm enough to commence the observations, for during the first fortnight strong westerly winds prevailed, and the observatory was occasionally buried in the drifted snow, when the only access to it was by digging down to the window of the room containing the clocks, and its mean temperature was only 6°, instead of the desirable one of 45°, at which the experiments in London had been conducted. During the second fortnight the mean temperature was 25°; but various difficulties occurred, and at the end of the month the thaw prevailed to such an extent as to oblige the abandonment of the house, before any satisfactory results had been obtained.

Towards the middle of June, therefore, the clocks were set up in a tent, which could be occasionally heated by a stove, and an account of their going is given from the 30th of June to the 14th of July, when it was conceived that a sufficient number of results had been obtained.

From the observations, fully detailed in this paper, respecting the length of the seconds' pendulum at the several places of observations, it appears that its length at London being, as ascertained by Captain Kater, =39.13929 inches, at Brassa it is 39.16929 inches, at Hare Island 39.1984 inches, and at Melville Island 39.207 inches.

Captain Sabine concludes this important communication with the following table, shewing the diminution of gravity from the pole to the equator, and the resulting ellipticity of the earth, deduced from the preceding observations by Captain Kater's method, described in the *Philosophical Transactions* for 1819.

	Dimunition of Gravity.	Ellipticity.
London and Brassa	.0055066	$\frac{1}{314{,}3}$
London and Hare Island .	.0055082	$\frac{1}{314{,}2}$
Brassa and Hare Island . .	.0055139	$\frac{1}{313{,}6}$
London and Melville Island	.0055258	$\frac{1}{312{,}6}$

2. *Some Observations and Experiments on the Papyri, found in the Ruins of Herculaneum. By* Sir Humphry Davy, Bart. P.R.S.

In this paper Sir Humphry Davy gives an account of his first experiments upon fragments of papyri, which induced him to hope that chemistry might afford assistance towards unrolling the MSS. He then describes those in the Museum at Naples, and the processes carried on there, and concludes with some general observations on the manuscripts of the ancients. The following is a brief outline of the interesting contents of this communication:

The papyri in question appear to consist of leaves reduced to the state of cinder, cemented by a matter soluble in certain liquids, and especially in muriatic and nitric ether; now, as chlorine, while it has a strong attraction for hydrogen, exerts no action upon carbonaceous substances, and as charcoal forms the basis of ancient writing-ink, it occurred to Sir Humphry that that gas might be usefully applied to destroy the adhesiveness of the layers; he therefore tried it and some other agents, possessed of analogous properties, and his attempts were to a certain extent successful.

The state of the Neapolitan specimens, and the general existence in them of undecomposed vegetable matter, suggest some curious remarks respecting the origin of the changes which they have suffered, and which have commonly been referred to the action of fire; the part of Herculaneum, however, in which they were found was not inundated by lava, but covered by sand and ashes, cemented together by the operation of water; and again, that fire is not necessary for the carbonization displayed by the manuscripts, is inferred from the state of the wood in the houses at Pompeii, which is always converted into charcoal, though covered by a shower of ashes, which must have been cold, as they fell at a distance of seven or eight miles from the crater of Vesuvius. Among the Neapolitan manuscripts there are some covered with a glossy substance resembling varnish, arising, Sir Humphry suggests, from the decomposition of the skin used to infold them, and now converted into a brilliant animal charcoal, leaving phosphate of lime when burned, but producing at the same time no inconsiderable quantity of ammonia.

One method only has been adopted for unrolling these fragile coils of carbonized papyrus; it consists in applying thin animal membrane (gold-beaters' skin), by a solution of glue to the back of the manuscript, and carefully elevating the layers by a silk thread, when the glue is dry. Alcohol and ether were found useful auxiliaries in this operation, and great advantage was also derived from throwing warm air upon the surface of the leaves, with precautions which are pointed out in the paper before us. The different MSS., however, required very different treatment.

During the two months that Sir H. Davy was employed in these experiments at Naples, he succeeded, with the assistance of the persons attached to the Museum, in partially unrolling twenty-three MSS., from which fragments of writing were obtained, and in examining about 120 others which gave no hopes of success:

> And I should gladly have gone on with the undertaking, from the mere prospect of a possibility of discovering some better results, had not the labour, in itself difficult and unpleasant, been made more so, by the conduct of the persons at the head of this department in the Museum. At first every disposition was shewn to promote my researches; for the papyri remaining unrolled were considered by them as incapable of affording any thing legible by the former methods, or, to use their own word, *disperati*; and the efficacy and use of the new processes were fully allowed by the Svolgatori or unrollers of the Museum: and I was for some time permitted to choose and operate upon the specimens at my own pleasure. When, however, the Reverend PETER ELMSLEY, whose zeal for the promotion of ancient literature brought him to Naples for the purpose of assisting in the undertaking, began to examine the fragments unrolled, a jealousy, with regard to his assistance, was immediately manifested; and obstacles, which the kind interference of Sir WILLIAM A'COURT was not always capable of removing, were soon opposed to the progress of our inquiries; and these obstacles were so multiplied, and made so vexatious towards the end of February, that we conceived it would be both a waste of the public money, and a compromise of our own characters, to proceed.

In respect to the date of these MSS., Sir Humphry remarks that, from the mixture of Greek and Roman characters, it is probable some of them were very ancient when buried. The ink with which they were written was a mixture of charcoal and glue, and from the omission of any mention by Pliny of an ink of galls and iron, it is not probable that it was used up to his period, but that parchment and our present writing ink were adopted together, " for a mixture of charcoal and solution of glue can scarcely be made to adhere to the skin; whereas, the free acid of the chemical ink partly dissolves the gelatine of the MSS., and the whole substance adheres as a mordant."

The earliest parchment manuscripts are probably the *Codices Rescripti*, discovered in the libraries of Milan and of Rome; in these, time has destroyed the vegetable matter of the ink, but solution of galls revives its blackness.

I have tried several substances for restoring colour to the letters in ancient MSS. The triple prussiate of potash, used in the manner recommended by the late Sir CHARLES BLAGDEN, with the alternation of acid, I have found successful, but by making a weak solution of it with a small quantity of muriatic acid, and by applying them to the letters in their state of mixture with a camel's hair pencil, the results are still better.

After all, we have probably sustained no great loss in the destruction of the Herculanean manuscripts; no fragments of Greek, and very few of Latin, poetry have been found in the whole collection, and the sentences which have been made out shew that the works are of the same kind as those before examined, and belong to the schools of the Greek Epicurean philosophers and sophists.

3. *Observations on Naphthaline, a peculiar substance resembling a concrete Essential Oil, which is apparently produced during the decomposition of coal-tar, by exposure to a red heat. By* J. Kidd, M.D., *Professor of Chemistry, Oxford.*

This is the very singular substance of which an account has already been given in this Journal (Vol. VIII. p. 287), but upon which no distinctive name had then been bestowed; the most important deficiency in its chemical history is unfortunately not supplied by Dr. Kidd. "With respect to the elementary constitution of this substance," he says, "I am not enabled to give any satisfactory information."

4. *On the Aberrations of Compound Lenses and Object Glasses. By* J. F. W. Herschel, Esq. F.R.S., &c.

In this elaborate and truly important paper Mr. Herschel presents, under a general and uniform analysis, the whole theory of the aberrations of spherical surfaces, and furnishes practical results of easy computation to the artist, disentangled from all algebraical complexity, and applicable, by interpolations of the simplest possible kind, to all the ordinary varieties of the materials on which he has to work.

5. *An Account of the Skeletons of the Dugong, two Horned Rhinoceros, and Tapir of Sumatra, sent to England by Sir Thomas Stamford Raffles, Governor of Bencoolen. By* Sir E. Home, Bart. V.P.R.S.

This memoir is illustrated by five plates, from drawings by Mr. Clift, without reference to which the details would scarcely be intelligible to the majority of our readers. Sir Everard justly eulogizes the exertions of Sir Thomas Raffles in promoting the pursuits of natural history and comparative anatomy; to his active interference we are indebted for the account of that very singular animal, the Dugong, published in the preceding volume of the *Philosophical Transactions*; it is the only one yet known that grazes at the bottom of the sea without legs; being of the figure and form of a whale, the position and structure of its mouth enables it to browse upon the fuci and submarine algæ like a cow in a meadow, and the whole structure of the masticating and digestive organs shews it to be truly herbivorous. It never visits land, or fresh water, but lives in shallow inlets, where the water is two or three fathoms deep. Their usual length is eight or nine feet. But a curious and, to some, perhaps the most interesting part of the history of this animal is, that the flesh resembles young beef, being very delicate and juicy; so that, as Sir Stamford, in the description of its dissection, remarks, "it afforded no less interest under the knife, than satisfaction on the table."

6. *On the Mean Density of the Earth. By* Dr. Charles Hutton, F.R.S.

We can never feel satisfied that it is fair to infer the density of the globe from that of any protuberance upon its surface, and are, therefore, not inclined to acquiesce in the importance of Dr. Maskelyne's researches, who aimed at ascertaining the mean density of the whole "terraqueous globe of our planet," by measuring the dimensions of an insignificant mountain in Scotland, and comparing its attraction on a plummet, with that of the whole earth on the same. Mr. Cavendish sought the solution of this problem by another method, consisting in ascertaining the attraction on small pendulous balls, of two inches diameter, by larger ones of ten inches diameter, as compared with the attraction of the earth on the same. The respective merits of these two experiments admit of considerable discussion, and upon these it is not our intention to enter; the learned author of the paper before us is in favour of the former, and he bore an active share in the investigations connected with it; he took all the measurements upon which the calculations respecting the magnitude of the mountain were founded, and, in short, performed all those computations recorded in the *Philosophical Transactions* for 1778, and which employed his daily labours during the greater part of two years. Under these circumstances he justly complains that his name has been withheld, with regard to the great share he took in those inquiries, and that assertions have been attributed to him which are not justified by any thing he has written. From a review of the investigations, he thinks it highly probable that the mean density of the earth is five times that of water, but not higher; Mr. Cavendish has assumed the mean density $=5.48$, a result, upon the accuracy of which Dr. Hutton has thrown much doubt. We do not think that the computations founded upon the mountain experiment, derive that verification from Mr. Playfair's *Lithological Survey* which Dr. Hutton is inclined to insist upon, and we wish that he had deduced the number 5. from more satisfactory data. When our author's computation of the earth's density was first made, the real density of the hill was unknown, "it was only known that it consisted chiefly of very hard and dense rocks," much heavier than common stone, which is allowed to be $2\frac{1}{2}$ times the density of water.

> I then, by way of example in applying the density, multiplied $\frac{9}{5}$ by $2\frac{1}{2}$, which produced $\frac{9}{2}$ or $4\frac{1}{2}$ for the density of the earth, on the smallest assumption; till such time as we should come to know more nearly what the real density of those rocks is: and therefore I must feel reason to complain, that this number ($4\frac{1}{2}$) has often been stated, rather unfairly, as my final conclusion for the earth's mean density; instead of being only the very lowest limit that might be used, till we could better learn something on that point with more certainty. But a lithological survey of the mountain being afterwards accurately made, at my earnest request, by that excellent philosopher and geologist, Mr. PLAYFAIR, the result of which was published in the *Philosophical Transactions* for the year 1811; I then applied his mean statement of the rocks to my own calculations, which gave me the number 5 for the density of the earth; as I published in the fourteenth volume of my edition of the *Philosophical Transactions*, and in the second volume of my *Tracts*.

In concluding his paper, Dr. Hutton suggests that one of the pyramids in Egypt might profitably be employed instead of a mountain for this experiment; such a body, he says, offers several advantages; its mass is sufficiently large, standing upon a base of about the size of the whole space of Lincoln's-inn Fields, and of a height almost double of that of St. Paul's Steeple.

> Then the station for the plummet, or zenith sector, could be taken much nearer the centre of the mass, than on a mountain, which would give a larger quantity of deviation of the plummet; then the regular figure and the known composition of the mass would yield great facilities in the calculation of its attraction; lastly, the deviation of the plummet might be ob-

served on all the four sides. Should such a project take place, it will be best to take the stations at about one fourth of its altitude above the base, that being the place where the deviation of the plummet would be the greatest. Finally, so favourable for such an experiment do those circumstances appear, and so anxious are my wishes for its completion and success, that, were it not for my great age and little health, I should be glad to make one in any party to undertake such an expedition.

7. *On the Separation of Iron from other Metals. By* J. F. W. Herschel, Esq.

The following is Mr. Herschel's mode of proceeding:

The solution containing iron is to be brought to the maximum of oxidation, which can be communicated to it by boiling with nitric acid. It is then to be just neutralized *while in a state of ebullition*, by carbonate of ammonia. The whole of the iron to the last atom is precipitated, and the whole of the other metals present (which I suppose to be manganese, cerium, nickel, and cobalt), remains in solution.

The precautions necessary to ensure success in this process are few and simple. In the first place, the solution must contain no oxide of manganese or cerium above the first degree of oxidation, otherwise it will be separated with the iron. It is scarcely probable in ordinary cases that any such should be present, the protoxides only of these metels forming salts of any stability; but should they be suspected, a short ebullition with a little sugar will reduce them to the minimum. If nitric acid be now added, the iron alone is per-oxidized, the other oxides remaining at the minimum. Moreover, in performing the precipitation the metallic solution should not be too concentrated, and must be agitated the whole time, especially towards the end of the process; and when the acid re-action is so far diminished that log-wood paper is but feebly affected by it, the alcaline solution must be added cautiously, in small quantities at a time, and in a diluted state. If too much alcali be added, a drop or two of any acid will set all right again; but it should be well observed, as upon this the whole rigour of the process depends, that no inconvenience can arise from slightly surpassing the point of precise neutralization, *as the newly precipitated carbonates of the above enumerated metals are readily soluble, to a certain extent, in the solutions in which they are formed (though perfectly neutral).* In the cases of cobalt and cerium, this re-dissolution of the recent precipitate formed by carbonate of ammonia is very considerable, and a solution of either of these metals, thus impregnated with the metallic carbonate, becomes a test of the presence of peroxide of iron, of a delicacy surpassing most of the re-agents used in chemistry, the minutest trace of it being instantly thrown down by them from a boiling solution, provided no marked excess of acid be present. To be certain however that we have not gone too far, it is advisable, after separating the ferruginous precipitate, to test the clear liquid, while hot, with a drop of the alcaline carbonate. If the cloud which this produces be clearly re-dissolved on agitation, we may be sure that only iron has been separated. If otherwise, a little acid must be added, the liquor poured again through the filter, so as to wash the precipitate, and the neutralization performed anew.

Such of our chemical readers as may be concerned in the analysis of ferruginous compounds, will find some remarks on the subject in the observations which follow the above extract.

8. *On the Re-establishment of a Canal, in the place of a portion of the Urethra, which had been destroyed. By* Henry Earle, Esq. *Surgeon to the Foundling*, &c.

The details of this paper are too purely surgical to admit of being quoted at length in this Journal, at the same time they lead to the demonstration of a curious and important physiological fact, and upon that ground the communication has probably been admitted into the *Philosophical Transactions.*

9. *Calculations of some observations of the Solar Eclipse of the 7th September*, 1820. *By* Mr. Charles Rumker.

The details of this paper scarcely admit of abridgment.

10. *An Account of the Remeasurement of the Cube Cylinder and Sphere used by the late Sir George Shuckburgh Evelyn, in his Inquiries respecting a Standard of Weights and Measures. By* Captain Henry Kater, F.R.S.

Sir G. S. Evelyn's experiments, adverted to in the title of this paper, are detailed in the *Phil. Trans.* of 1798, and though he bestowed the greatest attention on those parts of the inquiry relating to the *weight* of the solids, their *measurement* was not so accurately performed; Captain Kater therefore was anxious to re-investigate the latter subject, previous to the final report of the Commissioners of Weights and Measures.

In this paper, Captain Kater describes the state of the apparatus, and the means which he adopted in effecting their measurement. The mean result of the measurement of three sides of the cube gives for its contents 124.1969 inches.

The length of the cylinder, deduced from three means, is 5.9960 inches.

In the original measurement of the sphere, a brass square was employed, the side of which was a little larger than the diameter. The sphere being properly placed and supported within the square, a micrometer screw, which passed through one of the sides of the square, was brought in contact with the diameter of the sphere, and the reading of the micrometer head noted; the sphere being then removed, a brass ruler, of known length, was put into its place, and the micrometer screw being brought in contact with the end of the ruler, the difference between its length and the diameter of the sphere was obtained, from which the latter was determined.

Captain Kater details at length the repetition of Sir George's measurements, whence it appears that the excess of the diameter of the sphere above the length of the ruler gives 0.0012281 inches. The author then proceeded to measure the brass ruler, the length of which was found =6.0063609 inches, and the diameter of the sphere thence deduced gave 113.5264 inches for its solid contents.

Captain Kater concludes this paper with a table, shewing the data furnished by Sir G. S. Evelyn's experiments, and his own measurements, from which it appears that the weight of a cubic inch of distilled water, in a vacuum, at 62°, is =252.888 grs. of Sir George's standard, or 252.722 grains of the parliamentary standard.

11. *An account of Observations made with the Eight-feet Astronomical Circle, at the Observatory of Trinity-College, Dublin, since the beginning of the year* 1818, *for investigating the effects of the Parallax and Aberration on the places of certain Fixed Stars; also the comparison of these with former observations for determining the effects of Lunar Nutation. By* the Rev. John Brinkley, D.D., F.R.S. &c.

The observations, of which an account is given in this communication, were instituted with a view to discover the source of the differences that have existed between the observations made at Greenwich and those at Dublin, relative to parallax. After a perspicuous and detailed statement of the method of conducting his observations, Dr. Brinkley avows his inability to rectify the discordancies, observing, at the same time, that, as the whole extent of the difference in question is but one second, it may by some be considered as showing the great precision of modern observations. Yet, independent of the interest of the question of parallax, it is important that the origin of the difference, small as it is, should be ascertained.

12. *On the Effects produced on the Rates of Chronometers by the Proximity of Masses of Iron. By* PETER BARLOW, Esq., *of the Royal Military Academy.*

In a paper printed in the *Philosophical Transactions* for 1820, Mr. George Fisher, who accompanied Captain Buchan to the Arctic regions in 1818, has shewn that the rates of chronometers differ on board and on shore, gaining in the former, and losing in the latter situation; this he attributed to the magnetic influence of the ship's iron on the balance, and instituted various experiments, which with their results are detailed in his communication, to shew that the magnetic influence has a tendency to accelerate the motion of the time-keepers by its influence on the steel part of their balances. Mr. Barlow's inquiries contained in the paper before us have led him to different conclusions; he found, as might be expected, that the proximity of masses of iron influences the rates of chronometers, but so far from accelerating them, five out of six upon which he experimented were actually retarded, and the acceleration of the sixth was doubtful: he shews that much depends upon the *position* of the balance in regard to the iron. All these experiments prove that chronometers should be as carefully secluded from the action of any partial mass of iron, as the compass itself; and Mr. Barlow suggests, that as much of the iron of a ship is hidden, the best way of ascertaining a proper place for a chronometer will be to set down a compass in the situation designed for it, and to observe and compare the direction of its needle with that of an azimuth compass on deck, while the vessel is on different tacks, and if the disagreement between the two be considerable, to select another situation.

13. *On the Peculiarities that distinguish the Manatee of the West Indies, from the Dugong of the East-Indian Seas. By* Sir EVERARD HOME, Bart., V. P. R. S.

This is a short comparative description of the anatomy of these animals, illustrated by four plates from the accurate pencil of Mr. Clift. The first of these shews the external form of the Manatee, which has a broader tail and greater lateral extension of the ribs than the dugong; its habits of life place it between that animal and the hippopotamus; it has no tusks, and feeds upon plants growing at the mouths of large rivers. The second plate is a general view of the skeleton; the third, a representation of the stomach; and the fourth exhibits the peculiarities of its cæcum.

14. *On a new Compound of Chlorine and Carbon. By* RICHARD PHILLIPS, F. R. S. E., *&c., and* M. FARADAY, *Chemical Assistant in the Royal Institution.*

This compound was accidentally found by M. Julien, a nitric acid manufacturer of Abo, in Finland, amongst the products of his processes. It is a soft white tasteless solid; insoluble in water; volatile and inflammable; insoluble in acids and alcalis; and when raised in vapour over hot mercury, and detonated with excess of oxygen, chloride of mercury and carbonic acid are the results.

The analytical details of this paper prove the substance described to consist of one portion of chlorine = 33.5 and two of carbon = 11.4, so that it is intermediate between the protochloride and perchloride of carbon, previously described by Mr. Faraday. All endeavours to form this chloride, or to convert it into the other chlorides, have proved ineffectual; yet the smallness of the quantity of this compound in possession of the authors, prevented their pursuing this investigation to the extent they could have desired.

15. *On the Nerves: giving an Account of some Experiments on their Structure and Functions, which lead to a new Arrangement of the System.* *By* Charles Bell, Esq.

This paper, consisting of an inquiry into the functions of the nerves of the face, is preceded by some general observations on the nervous system.

If the views which Mr. Bell has presented in this communication be found to be correct, a very important addition is undoubtedly made to our knowledge of anatomy; and a part of the system hitherto very imperfectly understood assumes a character of simplicity and order in contrast not only with the doctrines which have been taught in our schools, but also with the opinions of the most celebrated foreign anatomists.

When we speak thus hesitatingly, we allude to the prefatory part of the paper; with regard to the facts and observations in the body of it, there can be but one opinion—they are quite new to us, and lead to the knowledge of phenomena important both in theory and practice.

Mr. Bell's leading principle of arrangement for the nerves of the *frame of the body*, is to divide them into two systems; one for the supply of common sensibility and the power of locomotion, to be found in all animals which have nerves, even in those very low in the scale of existence—while the presence of the other system depends on the complication of the structure of the animal, being complex in proportion to the number of the organs of the animal economy, and to the variety of functions which those organs perform. When an animal has no heart or concentrate organ of respiration, there will be none of those nerves found, which in the higher classes give so much intricacy to the whole system; but when these organs are present, then new nerves are bestowed upon the animal, and these will be simple or complicated, according as the relations of the superadded organs are few or many. For example, all animals that have a *heart*, *lungs*, and *stomach*, have a nerve appropriated to these organs, but this nerve will be found simple or otherwise, according to the functions performed by the viscera. If an animal merely breathes, the nerve will be simple in its distribution and in its connexions; but if the office of the lungs be multiplied, if they be employed in producing the natural cries of animals, or combine with the organs of the voice so as to produce articulate language, or with the organs of the face, so as to express emotions, then the simple respiratory nerve will become complicated, forming an intricate system of nerves for the combination of many remote parts in the acts of breathing, coughing, sneezing, hiccoughing, vomiting, *&c.*

This view, owing to its simplicity, is seducing enough, but its truth must be put to the test. The author brings the question very distinctly before us in explaining the variety of functions performed in the face.

In the head and face of man, and of the higher classes of brutes, there are two distinct sets of nerves which pass to the same parts. Anatomists have hitherto supposed that this twofold provision was for increasing the nervous power, and for securing its influence by numerous communications in parts liable to accident. But Mr. Bell, in opposition, as far as we know, to all anatomists and physiologists who have preceded him, has proved these two classes of nerves to be entirely different in functions.

By cutting across one of them he deprived the muscles of the face, and particularly of the nostril, of all power of motion in consent with the act of respiration; but this was not the only effect produced, for at the same time all that animal activity in the features which indicates passion, was destroyed. But it is very remarkable, that whilst those particular functions of the muscles of the face were suppressed by cutting one set of nerves, the muscles still retained their full power when employed in other functions, as, for example, in the act of eating. The next set of experiments is equally curious. Upon cutting across the other class of nerves, leaving that which controls the action of respiration entire, the sensibility of the skin and the action of the muscles in mastication were destroyed, whilst all those operations which we might suppose are the most delicate, as belonging to the act of breathing and expression, continued unimpaired.

We need scarcely add that these experiments were made upon brutes, but numerous examples of injury or disease of the nerves of the human face prove, that the same distinction or classification of functions exists in the human countenance.

We look forward with much interest to the promised paper on the Nerves of the Neck and Throat—and shall then endeavour to lay before our readers more detailed observations on the new views which Mr. Bell's investigations suggest to the physiologist, and on their probable importance in the practice of physic and surgery.

xvi. *Farther Researches on the Magnetic Phenomena produced by Electricity, with some new Experiments on the Properties of electrified Bodies in their Relations to conducting Power and Temperature. By* Sir H. Davy, Bart., P. R. S.

This is, in our opinion, by far the most important addition to the discoveries in electro-magnetism which has been made since the announcement of galvo-magnetism by Oersted; it is a clear and perspicuous, though brief narration, of several important researches, of which we need not say more than that they are worthy of the President of the Royal Society. Of these inquiries the following is a condensed abstract.

1. Magnetic phenomena are the same, whether electricity is small in quantity and passing through good conductors of considerable magnitude; or whether the conductors are so imperfect as only to carry a small quantity of electricity; in both cases the magnetism exhibited is extremely feeble.

2. Imperfect conducting fluids do not give polarity to steel when electricity is passed through them; but electricity passed through air produces this effect.

> Reasoning on this phenomenon, and on the extreme mobility of the particles of air, I concluded, as M. ARAGO had likewise done from other considerations, that the voltaic current in air would be affected by the magnet. I failed in my first trial, which I have referred to in a note to my former paper, and in other trials made since by using too weak a magnet; but I have lately had complete success: and the experiment exhibits a very striking phenomenon.

3. Metals are well known to be capable of transmitting large quantities of electricity; and one obvious limit to this quantity is their fusion by the intensity of the heat thus excited, which intensity is, of course, in part dependant upon the medium which surrounds them; thus, a platinum wire becomes much less heated by the transmission of a certain quantity of electricity when suspended in air, than when in the exhausted receiver of an air-pump. Reasoning on this fact, it occurred to Sir Humphry,

> That by placing wires in a medium much denser than air, such as ether, alcohol, oils, or water, I might enable them to transmit a much higher charge of electricity than they could convey without being destroyed in air; and thus not only gain some new results as to the magnetic states of such wires, but likewise, perhaps, determine the actual limits to the powers of different bodies to conduct electricity, and the relations of these powers.
>
> A wire of platinum of $\frac{1}{220}$, of three inches in length, was fused in air, by being made to transmit the electricity of two batteries of ten zinc plates of four inches with double copper, strongly charged: a similar wire was placed in sulphuric ether, and the charge transmitted through it. It became surrounded by globules of gas; but no other change took place; and in this situation it bore the discharge from twelve batteries of the same kind, exhibiting the same phenomena. When only about an inch of it was heated by this high power in ether, it made the ether boil, and became white hot under the globules of vapour, and then rapidly decomposed the ether, but it did not fuse. When oil or water was substituted for the ether, the length of the wire remaining the same, it was partially covered with small globules of gas, but did not become red hot.

To ascertain whether short lengths of fine wire, prevented from fusion by immersion in a cooling medium, transmitted the whole electricity of large batteries, a second independent circuit was so made from the ends of the battery by silver wires in water, that the decomposition of the water indicated the residuary electricity. It was thus found that an inch in length of platinum wire of one 220th of an inch diameter, kept cool by water, left a great residual charge of electricity in a combination of twelve batteries similar to those above mentioned; several trials shewed that it barely discharged six batteries.

4. Having therefore determined that there is a *limit* to the quantity of electricity which wires are capable of transmitting, it became easy to institute experiments on the different conducting powers of different metallic substances, and on the relation of this power to the temperature, mass, surface, or length of the conducting body, and to the conditions of electro-magnetic action.

The leading result of these researches was, that "*the conducting power of metallic bodies varied with the temperature, and was lower, in some inverse ratio as the temperature was higher.*" Thus a wire of platinum of one 220th of an inch diameter, and three inches long, discharged the electricity of two batteries when kept cool by oil; but when in air it barely discharged one battery; and some apparently paradoxical results depend upon this circumstance. Thus, let a fine wire of platinum be heated red hot in the galvanic circuit; then apply a spirit lamp to any part of it, so as to heat that part to whiteness; the consequence is, that the rest of the wire ceases to remain red hot, in consequence of the bad conducting power of the white hot portion: again, for the converse illustration, apply a piece of ice to a part of the wire, and the remainder, previously only red hot, will now become heated to intense whiteness, in consequence of the improvement in conducting power occasioned in the cool part, enabling a larger quantity of electricity to traverse the wire.

Another important fact developed in these researches relates to the great difference in the conducting power of different metals: in experiments with 6 inches of wire of $\frac{1}{220}$ inch diameter, silver discharged the electricity of 65 pairs of plates; copper and lead that of 56; tin of 12; platinum of 11; and iron of 9; all the wires being kept cool by immersion in water. These facts lead to others connected with the magnetic energies of the different wires which are not a little curious. If wires of the different metals be placed successively in the voltaic circuit the best conductors become most powerfully magnetic; that is, they take up the largest quantity of iron filings; so that in this way a silver wire becomes an infinitely more powerful magnet than one of iron. In a powerful voltaic circuit, (the wires not being capable of carrying off the whole of its electricity) two inches of silver wire of 1-30th inch diameter took up 32 grains of iron filings; a similar wire of copper only attracted 24 grains; of platinum 11; and of iron only $8\frac{2}{10}$.

There are various other important topics treated of in this paper; more especially the production of heat in various media by the transmission of electricity. That different wires become very differently heated when placed in the voltaic circuit was previously demonstrated by Mr. Children in the magnificent series of experiments which he instituted at Tunbridge with batteries of large plates, and of which an account is given in the *Philosophical Transactions* for the years 1809 and 1815*, and he ingeniously referred this variation of heat to the variable resistance opposed to the passage of electricity, assuming the elevation of temperature to be inversely proportionate to the conducting power. "The greatest heat, however," says Sir H. Davy, "is produced in air, where there is reason to suppose the least resistance, and as the presence of heat renders bodies worse conductors, another view may be taken; namely, that the excitation of heat occasions the imperfection of the conducting power. But till the causes of heat and of electricity are known, and of that peculiar constitution of matter which excites the one, and transmits or propagates the other, our reasoning on this subject must be inconclusive."

This is the concluding paper of the *Philosophical Transactions* for the year 1821. The hasty sketch of its contents in this and in our preceding Number will enable our readers to appreciate the novelty and importance of the papers printed in this volume; and will serve, we hope, as an index of reference to its valuable and multifarious information.

In taking our leave of this publication we seize the opportunity which it suggests of congratulating the members of the Royal Society upon the dignified independence and exalted station to which that learned body has attained, and which it promises to preserve with untarnished and even increasing splendour.

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* The facility with which an important discovery may escape the most vigilant observers is brought to our recollection by the multiplied attempts that were made during these experiments to influence the magnetic needle by the voltaic pile: they failed, because the separate poles only were presented to it; had the needle been at hand when the bar of platinum of 2½ inches length, and $\frac{276}{1000}$ inch diameter, was ignited by this powerful apparatus, the whole of the phenomena might have been developed, and under circumstances most favourable to their investigation.

ART. XV. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. VIII.

i. *Report on the present State of Ramsden's Dividing Engine. By Dr.* W. H. WOLLASTON. *Published by Permission of the* BOARD OF LONGITUDE.

SINCE the Dividing Engine belonging to the Board of Longitude appeared to Captain Kater and myself, from our examination of the divisions made by it, to be more defective than any other engine examined by the same means, we thought it desirable that this engine should be used a second time, with all possible care, to re-divide the double circle constructed for the purpose of this inquiry.

Mr. Cary readily undertook to repeat the operation, with attention to all the precautions that appeared necessary.

In the measurement of this second set of divisions, which I commenced shortly afterwards, I discovered that the divisions upon the outer circle, though made at the same time with those on the inner circle, and by the same motion of the cutting-point, were not coincident with them, as had been presumed to be the case in our former examination; and that it consequently became necessary to alter the mode of examination, which had been pursued under that presumption.

It is in fact the inner circle alone that really needs to be measured. For, since the motion of the cutting-point in marking the divisions is from the centre outwards, the inner circle must be regarded as a more correct copy of the engine than the outer, of which the divisions are liable to be misplaced by any irregularity, given to the motion of the cutter, in its passage over the line of contact between the inner and outer circle. In any examination by aliquot parts, (as by 6ths,) when any one arc on the outer circle has been compared with each arc of the inner in succession, and the differences measured, the sum of these differences divided by their number shows the error of the outer arc so compared, and thence the value of each inner arc, to which it has been compared, is ascertained.

In this method the number of measures, really necessary to be taken, is the same as before, but they occasion considerably more trouble than so many taken in one position of the instrument, from the necessity of placing the circles at a new point of coincidence previously to each measurement.

In my examination, however, I did not limit myself to the mere comparison of a single arc, but in each position I measured the errors of each aliquot part, so that, in fact, the number of measures actually taken, in an examination by 5ths, 6ths, 8ths, and 9ths, was $5^2 + 6^2 + 8^2 + 9^2$, the mean of which ensures greater accuracy in the results. The total amount of the errors on 22 points, obtained by this estimate of the divisions of this circle, after correction for common and central error, was found to be 78 seconds, or about 3½ seconds, on an average, for each division.

By subsequent examination of another set of points, beginning from 12° (instead of 0) in the same manner, I found a total of errors 95″, or 4⅓ on an average; so that the mean of both examinations shows this engine to be liable to an error of about *four seconds* on each division.

In consequence of the discovery of a difference between the inner and outer circles, it became necessary to re-examine the circle divided by Allan, but the measurements that I have taken do not invalidate our former result as to the comparative merit of his mode of constructing his dividing engine. The total amount of errors, in a set of 22 points, did not exceed 42″, making the average error of his engine less than *two seconds*.

The greatest error discovered on Allan's circle was 6½ seconds, while the greatest by Ramsden's engine is as much as 11″.

Specimen of the Examination of Cary's *Second Division by Ramsden's Engine.*

0.	0	72	144	216	288	Excess of Points on Outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
						—	+27.	+5.4	
72.	4.	1.	5.	10.	7.	27.			+5.4
							+12.	+2.4	
144.	3.	2.5	11.	14.	8.5	39.			+7.8
							+ 1.	+0.2	
216.	3.	8.	12.	13.	4.	40.			+8.0
							−27.5	−5.5	
288.	1.	3.	6.	5.	−2.5	12.5			+2.5
							−12.5	−2.5	
	36.	31.5	13.	8.	30.	36			0.

(Outer Circle.)

	+4.5	+18.5	+5.0	−22.	−6.
Errors of Arcs	+0.9	+ 3.7	+1.0	− 4.4	−1.2

Errors of Points . .	+0.9	+4.6	+5.6	+1.2
Do. Inner Circle . .	5.4	7.8	8.0	2.5
Diff. computed . . .	4.5	3.2	2.4	1.3
Do. observed Col. I.	4.	3.	3.	1.
	0.5	0.2	0.6	0.3

Specimen of the Examination of Cary's *Second Division by* Ramsden's *Engine.*

0.	0	60	120	180	240	300	Excess of Points on Outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
							—	+14.5	+2.4	
60.	3.	−4.	3.	8.	1.	3.5	14.5			+ 2.4
								+47.	+7.8	
120.	4.	4.	15.5	16.	10.	12.	61.5			+10.2
								−24.	−4.0	
180.	1.	5.	10.	13.	6.5	2.	37.5			+ 6.2
								−26.	−4.3	
240.	3.	0.	6.	9.	−3.	−3.5	11.5			+ 1.9
								− 7.	−1.2	
300.	1.	0.	6.	1.	−5.	1.5	4.5			+ 0.7
								− 4.5	−0.7	
	32.5	36.5	0.	−0.5	33.	28.	32.5			0.
(Outer.)		−4.	+36.5	+0.5	−33.5	+5.0	−4.5			
Errors of Arcs		−0.7	+ 6.1	+0.1	− 5.6	+0.8	−0.7			
Errors of Points . . .		−0.7	+ 5.4	+5.5	−0.1	+0.7				
Do. of Inner Circle .		2.4	+10.2	+6.2	+1.9	+0.7				
Computed diff. . . .		3.1	4.8	0.7	2.0	0.				
Observed do. 1st Col.		3.	4.	1.	3.	1.				
		0.1	0.8	0.3	1.0	1.0				

Allan—*Measured 5th February,* 1821.

0.	0	72	144	216	288	Excess of Points on outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
							−16.5	−3.3	
72.	0.5	7.	4.5	3.5	1.0	16.5			−3.2
							+17.	+3.4	
144.	2.	4.5	1.	−3.	−5.	− 0.5			+0.1
							+ 8.	+1.6	
216.	2.	3.5	−4.5	−7.	−2.5	− 8.5			+1.7
							+ 8.5	+1.7	
288.	0.	− 1.5	−7.	−4.5	−4.	−17.			+3.4
							−17.	−3.4	
	−8.0	−18.5	+2.	+7.	+8.0	−8.0			

(Outer Circle.)

	−10.5	+20.5	+5.	+1.	−16.
Errors of Arcs	− 2.1	+ 4.1	+1.0	+0.2	− 3.2
Errors of Points	−2.1	+2.0	+3.0	+3.2	0
Do. Inner Circle	−3.2	+0.1	+1.7	+3.4	
Diff. computed	1.1	1.9	1.3	0.2	
Do. observed Col. I. .	0.5	2.0	2.0	0	
	0.6	0.1	0.7	0.2	

N.B.—Since Allan's divisions are numbered from left to right, the sign + shows that the *outer* circle is in excess, and requires to be changed with reference to the inner circle.

Allan—*Measured 5th February*, 1821.

0.	0	60	120	180	240	300	Excess of Points on Outer Circle.	(INNER CIRCLE.)	Errors of Arcs.	Errors of Points.
								−17.5	−2.9	
60.	+1.	7.	3.	2.5	1.	3.	+17.5			−2.9
								+8.5	+1.4	
120.	2.5	5.5	3.	1.	−1.5	−1.5	+ 9.			−1.5
								+5.3	+0.9	
180.	2.7	4.5	−0.5	−1.	−4.	2.	+ 3.7			−0.6
								+3.4	+0.6	
240.	3.3	2.	−2.0	−3.5	−1.	1.5	+ 0.3			+0.0
								+1.7	+0.3	
300.	0.6	2.	−4.5	+0.5	−1.	1.	− 1.4			+0.3
	+0.5	−12.5	+11.0	+11.7	+14.3	+4.1	+0.5	−1.4	−0.2	0
(Outer Circle.)	−13.	+23.5	+0.7	+2.6	−10.2	−3.6				
Errors of Arcs .	− 2.2	+ 3.9	+0.1	+0.4	− 1.7	−0.6				
Errors of Points		−2.2	+1.7	+1.8	+2.2	+0.6	0			
Do. Inner Circle		−2.9	−1.5	−0.6	0	+0.3				
Diff. computed		0.7	3.2	2.4	2.2	0.3				
Do. observed Col. I. . .		1.0	2.5	2.7	3.3	0 6				
		0.3	0.7	0.3	1.1	0.3				

Results of the Measures of Cary's *second and best Division by Ramsden's Engine.*

	INNER CIRCLE.								
	(5)	(6)	(8)	(9)		Corrected by common Error —4·7.	Central Error 6 at 150°	Corrected for central Error.	
0						— 4.7	+ 5.2	+ 0.5	
40				3.6	+ 3.6	— 1.1	+ 2.2	+ 1.1	
45			2.2		+ 2.2	— 2.5	+ 1.5	— 1.0	
60		2.4			+ 2.4	— 2.3	0	— 2.3	
72	5.4				+ 5.4	+ 0.7	— 1.3	— 0.6	
80				7.5	+ 7.5	+ 2.8	— 2.1	+ 0.7	
90			10.4		+10.4	+ 5.7	— 3.0	+ 2.7	
120		10.2		9.6	+ 9.9	+ 5.2	— 5.2	0	
135			11.3		+11.3	+ 6.6	— 5.7	+ 0.9	
144	7.8				+ 7.8	+ 3.1	— 5.9	— 2.8	
160				5.7	+ 5.7	+ 1.0	— 5.9	— 4.9	
180		6.2	7.1		+ 6.6	+ 1.9	— 5.2	— 3.3	
200				8.1	+ 8.1	+ 3.4	— 3.8	— 0.4	
216	8.0				+ 8.0	+ 3.3	— 2.4	+ 0.9	
225			3.9		+ 3.9	— 0.8	— 1.5	— 2.3	
240		1.9		1.4	+ 1.6	— 3.1	— 0	— 3.1	
270			3.3		+ 3.3	— 1.4	+ 3.0	+ 1.6	
280				4.9	+ 4.9	+ 0.2	+ 3.8	+ 4.0	
288	2.5				+ 2.5	— 2.2	+ 4.4	+ 2.2	
300		0.7			+ 0.7	— 4.0	+ 5.2	+ 1.2	
315			—1.4		— 1.4	— 6.1	+ 5.7	— 0.4	
320				—1.4	— 1.4	— 6.1	+ 5.9	— 0.2	
360					108.6	68.2		36.2	=78″

Average $3\frac{1}{2}$ seconds.

Results of the Measures of Allan's *Circle, February,* 1821.

	INNER CIRCLE.					Corrected by common Error 0.6	Distance from 135°	Central Error 2. at 45°	Corrected for central Error.	
	(5)	(6)	(8)	(9)						
0						+0.6	45	+1.5	+2.1	
40				−3.9	−3.9	−3.3	85	+2.0	−1.3	
45			−3.2		−3.2	−2.6	90	+2.0	− .6	
60		−2.9			−2.9	−2.3	75	+1.9	− .4	
72	−3.2				−3.2	−2.6	63	+1.6	−1.0	
80				−2.1	−2.1	−1.5	55	+1.5	0	
90			−1.2		−1.2	−0.6	45	+1.4	+ .8	
120		−1.5		−1.8	−1.7	−1.1	15	+ .5	−0.6	
135			−1.5		−1.5	−0.9	0	0	− .9	
144	+0.1				+0.1	+0.7	9	− .3	+ .4	
160				−1.0	−1.6	−1.0	25	− .8	−1.8	
180		−0.6	−0.6		−0.6	0	45	−1.5	−1.5	
200				−1.1	+1.1	+1.7	65	−1.8	− .1	
216	+1.7				+1.7	+2.3	81	−1.9	+ .4	
225			+1.0		+1.0	+1.6	90	−2.0	− .4	
240		0		−0.1	−0.1	+ .5	75	−1.9	−1.4	
270			+0.9		+0.9	+1.5	45	−1.5	0	
280				+1.6	+1.6	+2.2	35	−1.1	+1.1	
288	+3.4				+3.4	+4.0	27	− .9	+3.1	
300		+0.3			+0.3	+ .9	15	− .5	+ .4	
315			−0.9		−0.9	− .3	0	0	− .3	
320				−1.2	−1.2	− .6	5	+ .2	− .4	
					34.2				19.	= 40″

Greatest $6\frac{1}{2}$ seconds: average less than 2 seconds.

ii. Mr. IVORY's *Method of computing the Latitude from two Altitudes and the Time intervening.*

Mr. IVORY, after a candid examination of the various approximations that have been proposed for the solution of the problem of two altitudes, is disposed to think them seldom so eligible as the direct computation, which he has somewhat abridged by a method that may be thus enunciated. (Phil. Mag. *Aug*. p. 84.)

Mr. Ivory's Rule.

Putting h the greater altitude.
h' the lesser,
D the mean declination,
t the half time elapsed, reduced to angular space,
(b the half base of the triangle A P B,
p the perpendicular,
y the perpendicular Z D falling from the zenith on p,
x the portion of p between y and A B,)
λ the latitude,
s the horary angle of the middle time,

$$A = \tfrac{1}{2}(\sin h - \sin h')$$

$$B = \tfrac{1}{2}(\sin h - \sin h')$$

1. $\operatorname{Sin} b = \cos D \sin t$
2. $\operatorname{Cos} p = \dfrac{\sin D}{\cos b}$
3. $\operatorname{Sin} y = \dfrac{B}{\sin b}$
4. $\operatorname{Cos} x = \dfrac{A}{\cos y \ \cos b}$
5. $\operatorname{Sin} \lambda = \cos y \ \cos(p \mp x)$
6. $\operatorname{Sin} s = \dfrac{\sin y}{\cos \lambda}$

Correction for the change of declination.

d the declination at B the greater altitude,

$\lambda - (D - d)\dfrac{\sin s}{\sin t}$ will be the true latitude,

$s + (D - d)\cos s\dfrac{\tan \lambda}{\sin t}$ the corrected horary angle.

DELAMBRE'S EXAMPLE. (*Astr. Coll.* No. 5.)

$h = 42° 14' 9''$, $h' = 16° 5' 47''$
$D = 8° 16' 30''$, $d = 8° 15'$
$t = 22° 30'$

Sin h	672184 (1)
sin h'	277214 (2)
2 A	949428
2 B	394940
A	474714
B	197470

L. cos D 9.995455 (3)
sin t 9.582840 (4)
sin $b = 22°15'11''$ 9.578295 (5)

L. sin b A. C. 0.421705
B 9.295501 (9)
sin $y = 31°25'44''$ 9.717206 (10)

L. cos y 9.931095
cos $(p - x)$ 9.945923 (14)
sin $\lambda = 48° 53' 5''$ 9.877018

L. cos b 9.966386 (6)
A. C. 0.033614
sin D 9.158134 (7)
cos p 81° 3' 14'' 9.191748 (8)

L. cos b 9.966386
cos y 9.931095 (11)
9.897481
A. C. 0.102519
A 9.676432 (12)
cos $x = 53° 3' 3''$ 9.778951 (13)
$p - x = 28$ 0 11

CORRECTION.

L. sin y 9.7172
cos. λ A. C. 0.1821
sin s 9.8993
sin t A. C. 0.4172
2.073 0.3165

$D - d = 90'$
2.073
$186.57 = 2' 47''$
48 53 5
48 50 18

Exceeding the truth by 18'' only.

iii. *Apology for the* POSTSCRIPT *on* REFRACTION, *in answer to Mr.* IVORY'S *Remarks.*

A mathematician of Mr. Ivory's acknowledged celebrity and transcendent attainments might naturally be expected, like the Midas of antiquity, to convert whatever he touches into gold:

but if he should employ his alchemical powers in converting the gold of others into brass, it would not be surprising if it should again be reported, as it was of old of the same Midas, that "*Auriculas asini Mida rex habet;*" and that the punishment should be remembered even longer than the alchemy.

Supposing for a moment that Mr. Ivory is right in condemning the series which has been employed for the computation of refraction, and supposing that his observations on the series "do not bear at all upon the table of refractions published in the Nautical Almanac for 1822; it is still scarcely credible that any mathematician, besides Mr. Ivory, will be hardy enough to assert that "the formula and the table must both be considered as *entirely empirical*." *Phil. Mag.* Sept. p. 167.

Can it possibly be believed that the most skilful empiric, even with the advantage of all the refined methods of empiricism that were cultivated by Lagrange, could ever have deduced, from a table of refractions only, a formula so complicated as this;

$$A = v\,\frac{r}{s} + \left(B + \tfrac{1}{2}v^2\right)\frac{r^2}{s^2} + C\,v\,\frac{r^3}{s^3} + \tfrac{1}{2}C\left(B + \tfrac{1}{2}v^2\right)\frac{r^4}{s^4}\ ?$$

The values of B and C only are determined empirically; but was Euler's theory of the moon entirely empirical, because some of the coefficients were determined from observation only?

Again, the only improvement, which the author of the table considers as of practical importance, is the introduction of an accurate and convenient mode of correcting for the heights of the barometer and thermometer. Will Mr. Ivory call this approximate correction *in any degree* empirical? He may assert that this computation is independent of the former: but it is, at least, an essential part of the table: a table on which even Mr. Ivory has condescended to bestow the conditional commendation, that "if it has a real and solid foundation, it must be allowed that no greater or more honourable testimony can be given in favour of—*the French Astronomers!*"

The French Astronomers really stand so little in need of this honourable testimony, that they will probably forgive the author of the table if he persists in withholding it; and its claim to the confidence of the public may safely be allowed to rest upon the acknowledged sufficiency of the French table, with which it agrees, in all ordinary cases, and upon its coincidence with the mean of 156 observations of Mr. Pond in low altitudes, as well as with Bradley's empirical correction for temperature near the horizon.

Nor would it be necessary to inquire whether or no the series is capable of being employed with convenience for an atmosphere of uniform temperature, in order that it may be "whispered into a pit" that an unjust judge has been *metamorphosed.* But the thing is really so easy, in comparison with other modes of computation, that it is not fair to the question to omit a further exemplification of the method proposed, with the assistance of the subdivision of the operation into two parts, which has been already suggested: and the whole may be resumed with little difficulty, from the first elements of the problem. We shall have, therefore,

x, the distance from the centre,
y, the superincumbent weight,
z, the density, here represented by y,

$$\mathrm{d}y = -my\mathrm{d}x; \ \frac{\mathrm{d}y}{y} = -m\mathrm{d}x; \ \mathrm{hl}y = m - mx.$$

u, the perpendicular to the direction,
$s = a^{\vee}$, the initial value of u,

$$u = \frac{s}{1 + pz} = \frac{s}{1 + py} = s - psy$$

$$\mathrm{d}u = -ps\mathrm{d}y$$

$$\mathrm{d}y = \frac{-\mathrm{d}u}{ps}$$

$$\mathrm{d}x = \frac{-\mathrm{d}y}{my}$$

$$v = \sqrt{(x^2 - u^2)}$$

$$\mathrm{d}r = \frac{\mathrm{d}u}{v}$$

$$\frac{\mathrm{d}u}{\mathrm{d}r} = v$$

$$\frac{\mathrm{d}x}{\mathrm{d}r} = \frac{\mathrm{d}u}{mpsy\mathrm{d}r} = \frac{v}{mpsy}$$

$$\frac{\mathrm{d}y}{\mathrm{d}r} = \frac{-v}{ps}$$

$$v^2 = x^2 - u^2 \,;\; v\mathrm{d}v = x\mathrm{d}x - u\mathrm{d}u$$

$$\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{x\mathrm{d}x}{v\mathrm{d}r} - \frac{u\mathrm{d}u}{v\mathrm{d}r} = \frac{x}{mpsy} - u \,;\text{ or } = \frac{x}{mpsy} - s$$

$$\mathrm{d}\,\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{\mathrm{d}x}{mpsy} - \frac{x\mathrm{d}y}{mpsy^2} - \mathrm{d}u$$

$$\frac{\mathrm{dd}v}{\mathrm{d}r^2} = \frac{v}{m^2p^2s^2y^2} + \frac{xv}{mp^2s^2y^2} - v\,;\text{ or } = \frac{v}{mp^2s^2y^2}\text{ only.}$$

$$\mathrm{d}\,\frac{\mathrm{dd}v}{\mathrm{d}r^2} = \frac{\mathrm{d}v}{mp^2s^2y^2} - \frac{2v\mathrm{d}y}{mp^2s^2y^3}$$

$$\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{1}{m^2p^3s^3y^3} - \frac{u}{mp^2s^2y^2} + \frac{2v^2}{mp^3s^3y^3}$$

$$\mathrm{d}\,\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{-3\mathrm{d}y}{m^2p^3s^3y^4} + \frac{2u\mathrm{d}y}{mp^2s^2y^3} + \frac{4v\mathrm{d}v}{mp^3s^3y^3} - \frac{6v^2\mathrm{d}y}{mp^3s^3y^4}$$

$$\frac{\mathrm{d}^4v}{\mathrm{d}r^4} = \frac{3v}{m^2p^4s^4y^4} - \frac{2uv}{mp^3s^3y^3} + \frac{4v}{m^2p^4s^4y^4} - \frac{4uv}{mp^3s^3y^3}$$

$$+ \frac{6v^3}{mp^4s^4y^4} = \frac{7v}{m^2p^4s^4y^4} - \frac{6uv}{mp^3s^3y^3} + \frac{6v^3}{mp^4s^4y^4}$$

$$\mathrm{d}\,\frac{\mathrm{d}^4v}{\mathrm{d}r^4} = \left(\frac{7}{mpsy} - 6u + \frac{18v^2}{psy}\right)\frac{\mathrm{d}v}{mp^3s^3y^3} - \left(\frac{28v}{mpsy} - 18uv\right.$$

$$\left. + \frac{24v^3}{psy}\right)\frac{\mathrm{d}y}{mp^3s^3y^4}$$

$$\frac{\mathrm{d}^5v}{\mathrm{d}r^5} = \left(\frac{7}{mps} - 6s + \frac{18v^2}{ps}\right).\left(\frac{1}{m^2p^4s^4y^4} - \frac{s}{mp^3s^3y^3}\right)$$

$$+ \left(\frac{28}{mpsy} - 18s + \frac{24v^2}{psy}\right)\frac{v^2}{mp^4s^4y^4}.$$

We shall find no difficulty in substituting for the coefficients of these formulas the numerical values employed by Laplace, and quoted by Mr. Ivory, that is $p = .000293876$ and $\frac{1}{m} = .00125254$; whence $\frac{1}{p} = 3403$, $\frac{1}{mp} = 4.2624$, $\frac{1}{m^2p^2} = 18.168$, $\frac{1}{mp^2} = 14505$, $\frac{1}{m^2p^3} = 61826$, $\frac{1}{mp^3} = 49\,360\,700$, and $\frac{1}{m^2p^4} = 210\,395\,000$.

The formula $ps\,\Delta\,y = vr + \frac{\mathrm{d}v}{\mathrm{d}r}\cdot\frac{r^2}{2} + \frac{\mathrm{d}^2v}{\mathrm{d}r^2}\cdot\frac{r^3}{2.3} + \ldots$ will then become

$$
\begin{aligned}
.00029388\,\Delta y = {} & v\frac{r}{s} + (2.1312 - .5\,s^2)\frac{{}^{2}}{s^2} + 2417.5\,v\frac{r^3}{s^3} \\
& + (2576 - 606\ s^2 + 4\,113\,400\ v^2\ )\frac{r^4}{s^4} \\
& + (12\,273\,040 - 2\,468\,000\,s^2 + 10\,570\,000\,v^2)\,v\,\frac{r^5}{s^5} \\
& + ([2\,045\,500 - 411\,333\,s^2 + 5\,260\,000\,v^2]\,.\,(4.2624 - s^2) \\
& + (8\,182\,000 - 1\,234\,000\,s^2 + 7\,013\,200\,v^2)\ 3403\,v^2)\frac{r^6}{s^6} \\
& + \ldots
\end{aligned}
$$

In order to compute the horizontal refraction in two portions from this formula, supposing its magnitude to be about .01, we may begin by taking half of this quantity, and make $r = .005$, and $r^2 = .000025$, s being $= 1$, and $v = 0$; we shall then have $p\,\Delta y = .00004578 + .000001231 + .000000082 + \ldots$

Now an important question is here to be considered; whether we are to content ourselves with simply adding together these terms as constituting the whole series, or whether it is justifiable to assume something more from analogy, or from probability, upon general principles, excluding all arbitrary conjectures depending on private reasons or imaginations.

If we admit the propriety of interpolating a table by the method of differences, which has seldom been called in question, it can hardly be denied that, with proper caution, the method of differences may very safely be applied to the continuation of a converging series. Supposing the progression to be very nearly geometrical, it cannot be questioned that there is a probability, approaching practically to a certainty, that we shall be much more correct if we suppose it to continue strictly as a geometrical progression, than if we break it off abruptly: and when the difference from a geometrical progression is more considerable, it seems at first sight natural to take the ratios or logarithms of the quotients, and to continue them by means of the differences. But where, as in the present instance, there are only three terms computed, and we have only one difference of these logarithms, the continual employment of this difference must be erroneous, because it will always ultimately lead us to a diverging series, affording the succeeding terms too large in proportion to the preceding. If, however, we compute one term only by this approximation, and suppose the remainder of the series to become geometrical, there will be a compensation of errors, which for ordinary purposes, will be amply sufficient, and which may often save us great labour in the computation of new terms.

Thus we have $\frac{45780}{1231} = 37.2$, and $\frac{1231}{82} = 15$; and $37.2 : 15 :: 15 : 6$, so that we may safely assume 6 as the quotient of a geometrical progression nearly equivalent to the proposed series, its sum being equal to the last given term divided by $6 - 1$, or here to .000000016, making the whole .00004711, which must undeniably be true to the last place, as far as the convergency of the series is concerned. From this value of $p \Delta y$ we have $\Delta y = .16031$, $y = .83969$, $\text{hl}\, y = - .174842$, and $x = 1 - \frac{\text{hl}\, y}{m} = 1 + .174842 \times .00125254 = 1.0002190$; but since $u = 1 - \Delta u = 1 - p \Delta y, = .99995289$, we have, for the new value of $s, \frac{u}{x} = .9997339$, and $v = .02307$; and proceeding with these values to compute $p \Delta y$, on the supposition $\Delta r = .005$, we have the series $.00011535 + .00004079 + .00000697 + .00000260 + .00000071 + .00000028 + [.00000040] = .00016710$, instead of $.00029388 - .00004711 = .00024677$, consequently .005 is much too little, and we must multiply the terms by the powers of $\frac{.006}{.005}$ or $\frac{.007}{.005}$, and the sums will become .0002188 and .0002802 respectively, giving by interpolation .006456 for r; but it will be necessary to repeat the operation with $r = .0065$, which gives us .0002453, and requires the further correction of .000025 only, making the whole refraction .011525 or 39′ 37″, which can scarcely differ above a second or two from the truth, the terms actually computed showing that it *must* be *less* than 39′45″, and the remainder being capable of a very sufficient estimation. Laplace's result is 39′ 54″,6; so that there must probably be some numerical error in one of the computations; at any rate the difference does not arise from the want of convergence of the series.

It may therefore be left with confidence to the decision of every impartial mathematician, to an Olbers, a Bessel, or a Brinkley, whether or no the convergence of the series is not sufficiently exemplified, to prove its utility for any computations of this kind that may be required. And even if four subdivisions had been employed, or twice four, instead of two, the labour of computation would still have been trifling, in comparison with the complicated expedients that are required in other methods.)

iv. *The Variation of the* TEMPERATURE *of the Atmosphere deduced from the mean* REFRACTION.

The best proof that the formula, published in the Nautical Almanac, is not deserving of the reproach of being merely *empirical*, will be found in the facility that it affords of deducing the actual density of the air, at a given height, from the table of astronomical refractions. For, by comparing the original series

$$p = v\frac{r}{s} + \left(\frac{\zeta}{mp} - s^2\right)\frac{r^2}{2s^2} + \left(\frac{\zeta' s}{mp} + \frac{\zeta v}{mp^2}\right)\frac{r^3}{6s^3} + \ldots,$$

with the expression in the Nautical Almanac, $p = v\frac{r}{s} + (2.47 + .5v^2)\frac{r^2}{s^2} + 3600\, v\frac{r^3}{s^3} + \ldots$, we obtain $\frac{\zeta}{2\, mp} = 2.97$, $\frac{\zeta' s}{6\, mp} + \frac{\zeta v}{6 mp^2} = 3600\, v$, $\frac{\zeta'}{mp} = 2484\frac{v}{s}$, $= 1.2855$, and $\zeta' = 537.6\frac{v}{s}$; and from these values we may immediately deduce the diminution of the temperature in ascending, and the rate at which that diminution varies at different heights, without any hypothesis whatever respecting the constitution of the atmosphere.

The number of feet, in which the temperature is depressed a degree of Fahrenheit, being called f, we have $\frac{1}{494 f}$ for the variation of density in a foot dependent on this cause; but the variation of the pressure y for a foot is $\Delta y = \frac{m}{20\,900\,000} = \frac{1}{27300}$; and this, if the temperature were uniform, would be the value of Δz, the increment of the density, which is actually only $\frac{1}{27300} - \frac{1}{494f}$; hence $\frac{\Delta z}{\Delta y} = \frac{dz}{dy} = \frac{1}{\zeta} = 1 - \frac{27300}{494f}$, $\frac{27300}{494f} = 1 - \frac{1}{\zeta}$, $494f = 27300 : \left(1 - \frac{1}{\zeta}\right)$, $f = 55.26 : \left(\frac{\zeta - 1}{\zeta}\right) = 55.26\left(\frac{\zeta}{\zeta - 1}\right) = 248$, at the surface of the earth ; and since $\zeta' = \frac{d\zeta}{dr}$ and $\frac{dz}{dr} = \frac{-v}{ps}$, we have $\frac{d\zeta}{dz} = \frac{-ps\zeta'}{v} = -537.6p$; whence, if we neglect the higher fluxions of ζ, we have for the point at which the density is reduced to one half, putting $\Delta z = -\frac{1}{2}$, $\Delta\zeta = 268.8\,p = .0759$; and ζ becomes 1.3614, giving $f = 208$ feet, for the elevation corresponding to a variation of temperature of 1° at the supposed altitude, which would probably be three or four miles above the surface. But, in order to compute the height from the density, or from the pressure, with greater convenience, it will be easiest to make the fluxion of y constant, so that we may obtain a formula for the height as depending upon the indication of the barometer. Thus, since $dx = -\frac{dy}{mz}$, $\frac{dx}{dy} = -\frac{1}{mz}$, $d\,\frac{dx}{dy} = \frac{dz}{mz^2} = \frac{dy}{mz^2\zeta}$, and $\frac{d^2x}{dy^2} = \frac{1}{mz^2\zeta}$, whence $d\,\frac{d^2x}{dy^2} = -\frac{2z\zeta dz + z^2 d\zeta}{mz^4\zeta^2} = -\frac{2z\,dy + z^2\zeta'\,dr}{mz^4\zeta^2}$, and $\frac{d^3x}{dy^3} = \frac{-2}{m\zeta^2} - \frac{\zeta'}{m\zeta^2} \cdot \frac{dr}{dy} = \frac{p}{m\zeta^3} \cdot \frac{s}{v} \cdot \zeta' - \frac{2}{m\zeta^2}$: consequently $\Delta x = -\frac{\Delta y}{m} + \frac{\Delta y^2}{2m\zeta} + \left(\frac{p}{6m\zeta^3} \cdot \frac{s}{v} \cdot \zeta' - \frac{1}{3m\zeta^2}\right)\Delta y^3$, $x = 1$ being the radius of the earth ; or, in feet, $\frac{\Delta x}{20\,900\,000} = -\frac{\Delta y}{766} + \frac{\Delta y^2}{1970} - \frac{\Delta y^3}{40371}$, and $\Delta x = -27300\,\Delta y + 10608\,\Delta y^2 - 518\,\Delta y^3$; or, if D be the descent of the barometer divided by the greater height, $H = 27300\,D + 10608\,D^2 + 518\,D^3$, since Δy is negative, and its square positive. Taking, for example, $D = \frac{1}{2}$, we have $H = 13650 + 2652 + 65 = 16367$ feet, or 3.1 miles, which appears to be not far from the truth: at Quito the barometer stands at 21.37, which gives a height of 8740 feet, instead of

9377; or if we augment the 8740 in the proportion of the expansion of air from 50° to 80°, it will become 9264; but it is well known that in computations of this kind it is necessary to introduce a variety of subsidiary corrections. The principal of these, however, is the reduction of the height of the barometer for temperature, and when this is applied, the result of the formula thus employed appears to be extremely accurate. Taking, for example, General Roy's observations on Moel Eilio, at the height of 2371 feet: the temperature, below, 68°, the corrected heights of the barometer 29.918 and 27.468, we have $D = .082$, and $H = 2306$, to which adding $\frac{18}{494}$, or 84, for the excess of the lower barometer above 50°, we have 2390, while General Roy's computations make the height 2393. In one of Mr. Greatorex's examples, the error of this method is 6 yards, while that of Dr. Maskelyne's is 12.

It may not be improper to observe that the correction of the refraction for temperature, as applied in the Nautical Almanac, may possibly be found to agree better with the mean temperatures of different climates than with the occasional variations at the same place, which may often be less regular in their causes and extent: and there is reason to think that, in some of these cases, the correction of $\frac{1}{400}$ for each degree of Fahrenheit, as employed by Bradley, is sufficient in the immediate neighbourhood of the horizon. Mr. Groombridge's observations, as published in the *Connaissance des Tems* for 1821, require the standard temperature of the table to be supposed 50°, when compared with the *exterior* thermometer, and not 48°, as the Greenwich observations seem to indicate. But it is at present impossible to expect any thing like perfect accuracy in a determination so liable to uncertainties of various kinds.

v. *Account of some Optical Inventions of* Professor Amici. *From the Memoirs of the Italian Society*, Vol. XIX.

1. The first, in importance, of Mr. Amici's papers is an account of an iconantidiptic telescope. Jeaurat's invention of a telescope that should exhibit at once two images, one erect, the other inverted, coinciding in the axis only, was improved by

Kratzenstein and Euler: but it was demonstrated by Boscovich that "an iconantidiptic telescope with three achromatic object glasses produces a much less effect than a common telescope of half the length," so that the advantage gained by the double velocity, with which the two images approach each other, is nothing more than might be obtained from a common telescope with twice or more than twice the magnifying power, as it might easily be made. He also showed that the telescope must fail in its proposed object of superseding the necessity of micrometer wires, for that the images could never be made to meet in the axis without having the assistance of a wire to guide the direction of their motion.

Mr. Amici observes that the optical difficulty might be avoided, by employing a reflecting instead of a refracting telescope, and by combining the constructions of Gregory and Cassegrain in the same instrument; but that the weight of such a telescope would render it inconvenient for astronomical observations. Still, however, the wires would be required, and the advantage of viewing very faint stars, without the danger of extinguishing their light by the illumination, would be lost. He therefore proposes to obtain a double image by reflecting half the rays, so that it may be formed in a simply inverted and wholly reversed position; and that if the plane of reflection be situated, for example, in that of the meridian, the images of all the stars in the field would meet each other at the true moment of their transit, while, in the construction of Jeaurat, none of them could coincide unless they were precisely in the axis of the tube. For this purpose he places, "in the focus of the eyepiece next to the object glass, a small rectangular isosceles prism of glass, the plane opposite to the right angle passing through the axis of the telescope, and its edges being perpendicular to it, so as to intercept half of the pencils of rays belonging to each point of the object, and to form with them a reflected image" depending on the internal reflection of the base of the prism. The instrument in this form appears to be peculiarly adapted to the observation of transits, though it may possibly require some modification both in the angle of the prism, and in its place with regard to the focus of the telescope.

2. The second memoir appears to exhibit a less fortunate attempt to obtain novelty without essential improvement. It relates to "the construction of an achromatic telescope without lenses, and with a single refractive medium." "It has hitherto been believed," says the author, "by natural philosophers, that the dispersion of colours is constant for the same refracting medium, or that a given refraction, produced by the same substance, is accompanied by a given dispersion. But I have found that the dispersion produced by more than one refraction is not by any means constant, but varies according to the various inclinations of the incident ray." He finds, however, that this property is really deducible from the constant proportion of the sines, and observes in conclusion, that "although the theory of colours has been cultivated by so many distinguished mathematicians and opticians, from the days of Newton to the present time, the property here described not only remained unknown, but the thing was judged impossible, until I discovered its practicability by means of some experiments which I was making with another view. We may therefore consider this circumstance as a striking proof, among many others, that in the prosecution of physical science, experiment is very often, and perhaps most commonly, more successful than theory, with regard to the development of all the circumstances that accompany a given phenomenon."

Now it is well known, that Euler was aware of the difference of dispersion that might be obtained in this manner from the same refractive substance, and the author himself quotes the work of our countryman, Dr. Brewster, who has entered very fully into the investigation of the subject. "The celebrated Dr. Brewster," he says, "in his excellent Treatise on New Instruments, informs us, page 400, that he has made several attempts to exclude colours by means of an object glass composed of two lenses of the same substance; but his experiments were not crowned with the desired success."

Mr. Amici, however, appears to have been considerably more successful in a practical point of view. He informs us that "ever since the year 1815, he has made telescopes of prisms of larger and smaller angles, which have fully answered his expectations. One of them, less than an inch in length, and half an inch in breadth, composed of little prisms of French glass, with angles of 45°, affords so much distinctness and precision in the outlines of the image, that it exceeds in its effect the most perfect achromatic opera glasses."

The construction of this extraordinary machine he has not more particularly described: he has, however, explained the general principle upon which it depends, and although it may be apprehended that it can never be applied to instruments of material importance, it really appears to have afforded him an elegant little plaything.

"Supposing, that through a prism, having its axis in a vertical direction, we look at a small square object, having one of its sides also vertical, it is obvious, that if we turn the edge of the prism so as to incline it towards the object, the image will become an oblong rectangle, instead of a square. If we then take a second prism of the same substance, and place it behind the former, with its axis horizontal, and turn it until it produces an equal deviation, the image will manifestly be prolonged in a vertical direction, and will again become a square, magnified, but still coloured. Now, since a coloured spectrum of a given extent may be produced in two different ways, that is, either by turning round its axis a prism with a small angle, in order to increase its refractive effect, or by making a prism of the same substance with a larger angle, it will be easy, without recurring to the first method, which would produce a distortion of the image, to determine the angle to be given to a third prism, in order that its least refraction may produce a spectrum of equal extent to that which is formed by the two combined prisms. If then we place this third prism behind the two former, in such a manner that its refraction may be in the direction of the diagonal of the square, it will correct the dispersion of the colours, without distorting the object, which will of course still remain magnified; so that the system of these three prisms alone will constitute an achromatic telescope consisting of a single refractive substance only."

CORRECTION of Art. v. No. VI. P. 376, Line 8 from the bottom, *for* "and consequently to the magnetic axis of the earth," *read* "and consequently to that of the dipping needle."

ART. XV. *Miscellaneous Intelligence.*

1. MECHANICAL SCIENCE.

§ MECHANICS, THE ARTS, &c.

1. *Letter from* Messrs. Parkinson and Frodsham, *to the Editor of the Quarterly Journal of Science and the Arts, relating to the Chronometers employed in the late Arctic expedition.*

SIR,

We take the liberty to address ourselves to you in the hope that our communication will be considered to fall within the plan of your Journal, which expressly concerns itself in the interests of science and of the arts.

We are the makers of the chronometers which have received so favourable a report in the Official Account of the late Arctic Expedition; confident of our workmanship, we ventured them on so severe a trial at our own expense, for the purpose of advancing our reputation; it will appear but reasonable, therefore, that we should be solicitous to possess that which we have been at much pains to obtain, and on which the prosperity of our business depends.

Soon after the return of the Expedition, it came to our knowledge that a report was in circulation, and had even obtained credit with gentlemen distinguished in science, that we were the sellers only and not the makers of the chronometers which had borne our name, and that Mr. Molyneux, previously known as a workman employed in making chronometers, was the real maker; we lost no time in obtaining and making public Mr. Molyneux's statement that he had never even seen the chronometers in question, and at the same time we asserted our own right to be considered the makers.

We hoped that we had thus terminated all doubts especially as there has been no other claimant than ourselves; it was therefore with equal surprise and concern that we read a note in the review of Captain Parry's *Voyage* in the 49th Number of the *Quarterly Review*, expressing an opinion that neither Mr. Molyneux nor ourselves, but some third person, (whom however the reviewer neither names nor specifies), was the real maker of the chronometers, the merits of which he was pleased to notice.

We might reasonably have complained that an opinion, affecting so materially our reputation and trade, should have been admitted in so respectable a work without at least more consideration and inquiry than had obviously been bestowed; we however overlooked this, and took the more moderate course of appealing to the right feeling and justice of the Editor himself in a letter which we have subjoined, requesting only that we might be permitted to prove either to himself, or to the writer of the note, that the opinion expressed in it was wholly erroneous.

We have experienced the additional mortification of finding our appeal entirely disregarded; our letter, which we trust was in all respects proper and respectful, has not received the common courtesy of a reply, and a subsequent Number of the *Quarterly Review* has appeared without notice or acknowledgment of error.

Our motive, in desiring the publication of our letter, is to remove the doubts which the *Quarterly Review* has excited to our prejudice, and to shew that our not having done so long since, has been solely occasioned by our reasonable trust in the readiness of the Editor himself to assist, when it should be shewn him that he had inconsiderately given currency to an opinion, injurious to a tradesman's reputation, and altogether without foundation.

We remain, Sir, respectfully,
Your most obedient humble servants,
PARKINSON AND FRODSHAM.

Change Alley, 26th Oct., 1821.

(COPY.)

To the EDITOR of the *Quarterly Review.*

Change Alley, July 1821.

SIR,

We beg leave to address you in consequence of the following Note in page 205 of the last Number of the *Quarterly Review.* "There is a dispute as to the real maker of these valuable chronometers. Mr. Molyneux, who has been long distinguished for the excellence of his workmanship, having set up a claim which is denied by Parkinson and Frodsham. As far as we are able to judge from their contradictory statements, we should say the real operator was neither of them but some third person."

The chronometers in question, Nos. 228, 253, 254, and 259, of our name, were sent by ourselves at our own risk and hazard on the Northern Expedition; our object was to manifest their goodness by a public trial under such extreme circumstances; we were personally unknown to the gentlemen in whose hands we placed them, but, as selected by the government on such an occasion, we did not doubt their care and disposition to do justice to our chronometers: the report which has been published of their going during the voyage has, in every respect, fully equalled our expectations.

We have stated thus much to you, in the hope that we may interest you to inquire into the justice of a note which has gone forth to the prejudice of our reputation and business, through the widely-extended circulation of the *Quarterly Review*; we have enclosed you the papers on which the author of the note states that his opinion has been formed, that the real operator was neither Mr. Molyneux nor ourselves, but some third person. Mr. Molyneux's acknowledgment that he never saw the chronometers, is decisive as to his concern in them. If we knew what third person can have been supposed the maker, we would meet the supposition by obtaining a denial from the individual himself.

We are ready, Sir, to give you, or the author of the note, any reasonable proof that may be required, that (with the exception of the springs of Nos. 253 and 254, which were obtained from Mr. Hopkins, whose distinct branch of the trade it is to supply springs to chronometer makers, and for which springs he received 2*l.* 10*s.* each), the four chronometers were our own workmanship, made in our own house, under our own direction, by workmen whom we have instructed, and that they were all finally adjusted and corrected by ourselves. We have laboured unceasingly since our entrance into life to become good artists, and to approve ourselves such to the public. The adventure of our property in the late voyage is a proof of our confidence in the excellence of our chronometers, and of our anxiety to establish our reputation. We cannot, therefore, but feel deeply when we see the credit which we have laboured to deserve, ascribed to some unknown person; and when we find the encouragement to our business, which we were receiving from the public, withheld in consequence of the promulgation of an opinion which has certainly no foundation in fact.

We hope that you will not refuse to satisfy yourself of the truth of this statement.

We are, Sir,
Your obedient servants,
PARKINSON AND FRODSHAM.

2. *Steam Engines of England.*—M. Dupin, whilst speaking of the immense mechanical force set in action by the steam engines of England, gives the following illustration of its amount. The great pyramid of Egypt required for its erection the labour of above 100,000 men for twenty years: but if it were required again to raise the stones from the quarries and place them at their present height, the action of the steam engines of England which are managed at most by 36,000 men, would be sufficient to produce the effect in eighteen hours. And M. Dupin says, that if it were required to know how long a time they would take to cut the stones and move them from the quarries to the pyramid, a very few days would be found sufficient.

The calculation of M. Dupin is as follows: the volume of the great pyramid is 4,000,000 cubic metres, its weight is about 10,400,000 tons, or 10,400,000,000 kilogrammes. The centre of gravity of the pyramid is elevated 49 metres from the base and taking 11 metres as the mean depth of the quarries, the total height of elevation is 60 metres, which multiplied by 10,400,000 tons gives 624,000,000 tons raised 1 metre. Then the total of the steam engines in England represents a power of 320,000 horses. These engines, moved for 24 hours, would raise 862,800,000 tons 1 metre high, and consequently 647,100,000 tons in 18 hours, which surpasses the produce of labour spent in raising the materials of the great pyramid.

3. *Prize Question.*—When the nozzle of a blowing machine is placed at a certain distance from that of the tuyère, a stronger current of air is obtained than when both are placed together, as is frequently done. This effect is produced by various causes dependant on the elastic nature of the fluid in motion, and of the surrounding atmosphere. The Society of Sciences and Arts at Metz have founded the following prize question on this experiment: "What are the changes necessary to be made in the tuyère of blowing machines, to introduce, in the most advantageous manner, the good effect indicated above, or any other improvement for the rapid transmission of air to greater or smaller distances." The prize is 300 francs, and is to be adjudged in April, 1822.

4. *Prize Question.*—The Royal Academy of Sciences of Toulouse has proposed as the subject of prize essays, "A physico-mathematical theory of drawing and forcing-pumps, stating the ratio between the moving power and the quantity of water elevated; attention being given to all the obstacles which the force has to overcome." Among these obstacles are enumerated, the weight and inertia of the column of water, its friction against the tubes, its contraction at the apertures of the valves, the weight and friction of the pistons, the weight of the valves, the inequality between the upper and lower surface at the moment the pressure opens them, &c." The papers are to be written in French or Latin, and sent in before May, 1823. The prize is a gold medal of 500 francs value.

5. *Lithography.*—An experiment has lately been made to take off impressions from plants by lithographic printing. A specimen of *Sibthorpia Europæa,* which was gathered several years ago in Cornwall, was covered with lithographic ink and impressed on a stone, from which stone several impressions were afterwards taken. The experiment was not so successful as was wished, but still promised to be beneficial in leading to the means of multiplying copies of the impressions of plants,

much more accurate, in some respects, than a drawing can be expected to be.—*Phil. Mag.*, Sept., 235.

6. *Preservation of Milk.*—The following method is recommended for the preservation of milk, either at sea or in warm climates. Provide pint or quart bottles, which must be perfectly clean, sweet, and dry; draw the milk from the cow into the bottles, and as they are filled, immediately cork them well up, and fasten the corks with packthread or wire. Then spread a little straw on the bottom of a boiler, on which place the bottles, with straw between them, until the boiler contains a sufficient quantity. Fill it up with cold water; heat the water, and as soon as it begins to boil, draw the fire, and let the whole cool gradually. When quite cold, take out the bottles, and pack them with straw or saw-dust in hampers, and stow them in the coolest part of the ship, or in a cool place. Some years since, there was a Swedish or Danish vessel at Liverpool, having milk on board preserved in this manner; it had been carried twice to the West Indies and back to Denmark, and had been above eighteen months in the bottles; nevertheless, it was as sweet as when first milked from the cow.—*New Monthly Mag.*, 316.

7. *Preservation of Cauliflowers.*—These vegetables have been preserved two or three months by digging a trench under a wall, eighteen inches wide and deep, laying in the cauliflowers with the stems inclined upwards, and covering the whole in with earth, heaping up the surface in an inclined form, so that the rain should run off.

8. *Use of Larch Bark in Tanning.*—Mr. E. Smith, from repeated trials made by himself and friends, strongly recommends the use of larch bark in tanning, not only for light calf, deer, or sheep's skins, but for stout hides; and states, that sole-leather tanned with it, and worn against other leather tanned with vallonia, resisted the wear better, and did not imbibe so much water. He then asks whether there is any further occasion for the importation of Dutch or German bark.

9. *Atkin's Rock.*—The dangerous ledge of Atkin's Rock has been marked and observed very accurately by Captain Cork, of the Barnet, from Demerara to Liverpool. Its position has not been determined exactly, but the Captain announces its situation to be precisely 50° 5′ latitude, and 12° west longitude from Greenwich.

10. *Preservation of Fresco Painting.*—A new process for removing frescoes from one wall to another without in-

jury to the painting, has been devised by Signor Steffano Barezzi, of Milan. The picture is covered with a prepared canvass to which it adheres, and is thus detached from the wall. The canvass is afterwards applied to another wall, to which the painting again attaches itself without the least trait being destroyed. The practicability of this method has been successfully proved, and the inventor is now employed in transferring a large fresco from the church Della Pace, at Rome. Great expectations are entertained that he will be able thus to rescue from destruction the celebrated Cena of Leonardo da Vinci.—*Magazine of Fine Arts*, 474.*

11. *Green Paint.*—Gas tar, mixed with yellow ochre, makes an excellent green paint, very useful for preserving coarse wood work or other articles.

II. CHEMICAL SCIENCE.

§ CHEMISTRY.

1. *Curious effect of Sea Water upon Cast Iron.*—In a recent visit to Plymouth, Mr. Hatchett obtained from Mr. Whidbey, a portion of a cast-iron gun, which had long been immersed in sea water: it was incrusted to the depth of an inch with a substance having all the exterior characters of impure plumbago; easily sectile, greasy to the touch, and leaving a black streak upon paper. This substance, digested in water, afforded a small quantity of muriate of iron, but was not otherwise affected. Digested in muriatic acid, a considerable portion was dissolved without any effervescence; and the solution had the properties of pure muriate of iron, with a trace of manganese. The insoluble portion, when collected upon a filter, washed, and dried, was a shining black powder, very soft and unctuous to the touch, and apparently pure plumbago. At Dr. Wollaston's suggestion, I examined it for manganese: I deflagrated a portion of it with chlorate of potassa, dissolved the residue in muriatic acid, and threw down oxide of iron by pure ammonia; the neutral solution was filtered, and evaporated to dryness; the dry salt was entirely dissipated by heat, and gave no trace of manganese. The relative proportions of the component parts of this substance were,

Oxide of iron - -	81
Plumbago - - -	16
	97

Anchors, and other articles of wrought iron, when similarly exposed are only superficially oxidized, and exhibit no other peculiar appearance. There can, therefore, be little doubt that the rapid decay and change of cast iron is partly to be attributed to a galvanic action, the plumbaginous crust in contact of the cast metal producing an electro-motive combination, aided by and promoting the decomposition of the sea water, and of its saline contents. Considering the resemblance of this crust to that described by Mr. Daniell, as produced by the action of acids upon cast iron, (see Vol. II., p. 283, of this Journal), I expected to detect a portion of silica in it, but was disappointed.

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* Is not the "Last Supper" of L. da Vinci already destroyed? ED.

2. *Solubility of Magnesia, and its Carbonate in Water.*—Dr. Fyfe has lately experimented on the solution of magnesia and its carbonate in water; and not only finds that these substances are soluble, but also that, as with lime, they are more soluble in cold than in hot water. The magnesia was prepared by precipitation, washing, and ignition; water was then digested on it, and afterwards the magnesia, dissolved in the water, separated by carbonate of ammonia and phosphate of soda. In this way it was ascertained, that 1 part of magnesia required 5142 parts of water, at a temperature of 60°, to dissolve it; and at a temperature of 212°, 36000 parts of water. The effect of temperature is so great in altering the solubility of magnesia in water, that if a cold saturated solution be heated in a narrow-necked matrass, it becomes turbid on rising to 212°.

On repeating the experiments with carbonate of magnesia, it was found that 1 part required 2493 parts of water, at the temperature of 60°, for its solution, and 9000 parts, at the boiling point.—*Edinburgh Journal*, vol. v., p. 305.

3. *On the Solution of Carbonate of Lime, by* Mr. Dalton.—Mr. Dalton, in a paper containing remarks on the analysis of spring and mineral waters, has stated some interesting circumstances respecting the alkaline properties of solution of carbonate of lime. It is stated, " that all spring water containing carbonate or super-carbonate of lime, is essentially limy or alkaline, by the colour tests. And this alkalinity is not destroyed till some more powerful acid, such as the sulphuric or muriatic, is added, sufficient to saturate the whole of the lime. Indeed, these acids may be considered as sufficient for tests of the quantity of lime in such waters; and nothing more is required than to mark the quantity of acid necessary to neutralize the lime. It does not signify whether the water is boiled or unboiled, nor whether it contains sulphate of lime along with the carbonate; it is still limy in proportion to the quantity of carbonate of lime it contains. Agreeably to this idea, too, I find that the metallic oxides, as those of iron or copper, are thrown down by common spring water, just the same as by free lime. Notwithstanding, this carbonate of lime, in solution in water, contains twice the acid that chalk or limestone does. I fully expected the super-

carbonate of lime in solution to be acid; but it is strongly alkaline, and scarcely any quantity of carbonic acid water put to it, will overcome this alkalinity. Pure carbonic acid water is, however, acid to the tests. I could not be convinced of the remarkable fact stated in this paragraph, till I actually formed super-carbonate of lime, by super-saturating lime water in the usual way, till the liquid from being milky became clear. It still continued limy, and was even doubtfully so when two or three times the quantity of acid was added. It should seem, then, to be as impossible to obtain a neutral carbonate of lime, as it is to obtain a neutral carbonate of ammonia, in the sense here attached to the word neutral."—*Memoirs of the Manchester Society.*

4. *On Sulphureous Mineral Waters, and the Nitrogen contained in them.*—The existence of nitrogen in mineral waters has frequently been ascertained, and its quantity stated; but no very precise ideas have hitherto been published, on the cause of its existence in the waters, and the attempt of M. Anglada is, perhaps, the first made to shew its source, and the situations in which it may be expected. Having observed from the experience of others and himself, that nitrogen occurred in waters containing sulphuretted hydrogen, he was induced to search whether it was not constantly present in such waters, and finding that to be the case, reasoned and experimented upon its production. In many sulphurous springs, the sources of which were easily arrived at, it was readily observed, that nitrogen either rose from the sides of the spring, or could be obtained without difficulty from the water; but in others, which were confined by pipes or conduits, it was necessary to open up the works, and get to the source. In all cases, however, sulphurous waters were found to contain nitrogen; and it was remarked, that though the water contained abundance of sulphuretted hydrogen, the gas contained none.

M. Anglada expected by heat to obtain sulphuretted hydrogen and carbonic acid gas from sulphureous water; but in no case obtained any thing except pure nitrogen gas. Guided by this result, he concluded that, in all the sulphureous waters that had thus yielded him nitrogen, that gas came from the air which the waters had taken up in their subterraneous course, and from which the oxygen had been separated by the sulphur, &c. In order, as it were, to prove this opinion, a portion of a sulphureous water was treated with acetate of lead, to separate all the sulphuretted hydrogen from it, and then boiled: it gave out more gas than before, and the gas was a mixture of oxygen and hydrogen.

The air thus furnished to waters, is given to them, M. Anglada thinks, in the bowels of the earth, by currents of air of which we know little or nothing.

In consequence of this action of the air on waters of this kind, it happens sometimes that a water decidedly sulphureous at its source, ceases to be so at a little distance from it; dependent on the strength of the water, and the means it has of getting air. In those cases, the previous sulphureous state of the water may be deduced from the disengagement of pure nitrogen, or containing very little oxygen, and from certain glairy appearances which are exhibited by those waters.

M. Anglada also remarks, that in many waters containing carbonic acid, abundance of nitrogen is found, and he suggests, that probably the air may act in them on some carbonaceous matters, giving rise to carbonic acid and nitrogen at the same time. It is also remarked in the conclusions attached to this paper, and which being given above, need not be repeated, that the change takes place at all temperatures, and that if nitrogen is not found in every sulphurous water, it is in all those containing a hydro-sulphuretted alkali.—*Ann. de Chim.*, vol. xviii. p. 113.

5. *On Meteoric Iron, and the Masses supposed to be such.*—In the year 1810, whilst examining a specimen of graphite from Saxony, Mr. J. F. John discovered nickel in it, and afterwards ascertained the presence of cobalt in a mass of iron, similar to that of Pallas, but found in an old collection. These circumstances led him to consider that the opinion generally entertained of some characters of meteoric iron, or meteorolites, were incorrect. It has been assumed that any stones or pieces of iron, supposed to be meteoric, and containing nickel, to the exclusion of all other metals, were certainly of the origin supposed. The uncertainty of this test, and the remembrance that M. Laugier had discovered chromium in some aërolites, and also that the occurrence of this metal had been proposed as a test of the meteoric origin of stones, &c., induced M. John again to analyze portions of the large masses of iron considered as meteoric, and also portions of iron taken by the magnet from meteorolites. The following are the results of these analyses:

	Iron of Pallas.	Iron of Ellbogen in Bohemia.	Iron of Humboldt.
Iron	90	87.5	91.5
Nickel	7.5	8.75	6.5
Cobalt	2.5	1.85	2
Chromium	trace	and loss 1.9	trace.
Manganese	0		0
	100	100	100

The mass of Aix-la-Chapelle contains iron, a little arsenic, traces of charcoal and of cobalt, and perhaps of sulphur. The mass taken from beneath the pavement in 1762 is evidently the produce of art. Polyxene, or the black grains found in a particular sort of platina, are simply metallic iron, without any mixture of other metals. The iron from Portuguese America, composed of crystalline grains, and which is very rare, contains a little copper and cobalt, and perhaps a trace of chromium and manganese. This latter iron was obtained from M. Krusenstern, and M. John found a similar kind in the mineralogical collection of Zurich, which, according to the table, had been brought by M. Peterson from Kamtschatka.

Having pulverized some meteoric stones, and obtained the iron from them by means of a magnet, M. John analyzed it for the purpose of comparing it with the analyses already mentioned. The results were as follows:

	Iron from the Aerolite of Chatonnay.	Of l'Aigle.	Of Sienne.
Iron	92.72	92.72	92.72
Nickel	5.5	5.5	5.1
Sulphur	1	Quantities too small to be weighed.	
Cobalt	0.78		
Chrome	trace		

The conclusions drawn from these experiments are, 1. That the iron of meteoric stones and of the large ductile masses mentioned, contain the same substances; namely, iron, nickel, cobalt, chrome, and perhaps also manganese, as in that from Ellbogen. 2d. Iron of meteoric stones contains less nickel than the large masses of soft iron. 3. Iron of meteoric stones evidently contains sulphur, but probably not in combination with the whole of it, but forming magnetic pyrites disseminated through it. The large masses found in the earth contain no sulphur.—*Ann. de Chim.* xviii. p. 198.

6. *Tests for Arsenic.*—Dr. Porter, of the University of South Carolina, in observing on the tests for the detection of arsenic, remarks, that an appearance, similar to Scheele's green, is produced by carbonate of potash, added to a solution of copper containing coffee, but without arsenic, more striking than if a weak solution of arsenic be used. He also states that, in the production of Scheele's green by arsenic, sulphate of copper and carbonate of potash, chromate of potash might be substituted for the arsenic; and that the precipitate produced could not be distinguished by the eye from Scheele's green. Also that Mr. Hume's test of the nitrate of silver (as modified in its application by Dr. Marcet,) gave, with chromate of potash, a yellow precipitate, which, when placed side by side with one pro-

duced by arsenic, could not be distinguished by colour or appearance from it.—Silliman's *Jour.* iii. p. 355.

7. *On the detection of Arsenic in Ores of Antimony.*—The following is the process of M. Serulas, by which he detects arsenic in antimony. The antimony, or its oxide, is mixed with cream of tartar, and heated in a close crucible: in this way an alloy of the antimony with potassium is formed. The alloy is then placed under a bell-glass filled with water, and a large quantity of hydrogen is soon liberated. This, when burned in a narrow glass jar, deposits a brown pellicle on its sides, if the antimony contains arsenic, but no pellicle is produced if arsenic be not present. Very small quantities may be detected in this way.

8. Berzelius *on the Oxide of Platinum and Gold.*—After remarking on the numbers given for the oxides of platinum by Mr. Cooper and Dr. Thomson, M. Berzelius proceeds to describe the process he now adopts to obtain these numbers accurately. The muriate of platina and potassa being a neutral compound, a portion of it was well washed and dried; it was then anhydrous, and by heat gave off chlorine, without any trace of water; so that it may be concluded to be a double chloride of platinum and potassium. ×2.251 grains were slowly heated in a current of dry hydrogen as long as muriatic acid was formed. It lost .65 of a grain in weight, and the residue washed left .898 of platinum undissolved. It was therefore composed of

Chlorine	.650
Platina	.898
Chloride of potassa	.703

In the tables of M. Berzelius the numbers are such that, if calculated, it will be found that the platinum contains twice as much chlorine in the above compound as the potassium. An atom of muriate of potassa is 1865.13, and 2 atoms of muriate of platina are 4201.6, so that the compound atom would weigh 6066.19. This would give in 2.251 grains of the substance .657 chlorine, .898 platina, and .696 muriate of potassa, which is nearly accordant with the experiment above.

The same results were obtained with the muriate of soda and platinum. This salt contains 1 atom of muriate of soda, 2 atoms muriate of platinum, and 12 atoms of water.

M. Berzelius still retains his opinion of the number for gold, or the weight of its atom (2486.) He adds an experiment in which he reduced it by phosphorous, but says it is not so precise as the reduction by mercury, consequently it adds nothing in proof of the correctness of the numbers given. M. Pelletier, it will be remembered, gives the number of gold as 2993.

M. Berzelius describes two muriates of gold. The super-muriate crystallizes readily in small needles, of a pale-yellow colour; it is less soluble in water than the neutral muriate, and its solution is of a gold-yellow colour. The neutral muriate is obtained by exposing the super-muriate to a low heat. It loses first muriatic acid, and then a mixture of it with chlorine, so that it begins to decompose even before all the free acid is driven off. To obtain it perfectly neutral, it must be prepared by adding hot water to the proto-muriate of gold, which is then reduced to gold and the neutral muriate. Its solution is of an intense ruby colour; it yields a reddish-brown saline mass, which liquifies in the air. The addition of muriatic acid changes the colour to yellow, and per-muriate crystallizes, if the solution is concentrated.—*Ann. de Chem.* xviii. p. 146.

9. *On rendering Tissues incombustible.*—M. Gay-Lussac mentions, as the result of his experiments on the best means of rendering cloth incombustible, that solutions of muriate, sulphate, phosphate, and borate of ammonia, with borax, and some mixtures of these salts, were found the most effectual; in fact, according to his theory on the subject, those substances which at the temperature of burning best shielded the fibre from the air, and supplied most incombustible gas or vapour to the flame, proved to be most efficacious.

10. *Heat produced by Chlorine.*—Among the obvious qualities of chlorine, Dr. Silliman mentions the heat it excites on the hand when plunged into it. The effect is equal to a temperature of 90° or 100°.

11. *Explosion of Chlorine and Hydrogen.*—Dr. Silliman cautions chemists against making mixtures of chlorine and hydrogen in large quantities without due precaution. He relates some instances in which two or three quarts of the gases had been mixed, where the explosions produced on exposure to sunlight were instantaneous and very powerful, and one also in which the explosion took place by exposure of the gases to a very diffuse and dim light.

12. *Oxalic Acid.*—M. Berzelius decomposed the oxalic acid by first forming an oxalate of lime of determinate composition, as to the water it contained, and then digested it with muriate of gold. The gold was reduced, and carbonic acid formed. 4 grains of oxalate, containing 3.25 of the anhydrous compound, reduced 3.352 grains of metallic gold, from which the composition of oxalic acid is deduced as 2 atoms of carbon and three atoms of oxygen, no hydrogen being present. This result was confirmed by the action of the quadroxalate of potassa on solution of gold; 2 grains of it precipitate 2.05 of the metal.

13. *Alum in decomposed Mica Slate.*—Among the sources of alum there is one which we have never seen mentioned by any author. It is from the decomposition (not of clay-slate, but) of mica-slate. We have been frequently presented with specimens of alum found in decomposed mica slates, as from the towns of Preston, Waterbury, and Huntington in Connecticut, and indeed we have seen but few instances of American alum derived from any other source. It is, as we are informed, so abundant in some places, that the people use it in dyeing without resorting to any other supply. We are not aware that this source of alum has been observed in other countries.—*American Journal*, iii., p. 240.

14. *American Hydraulic Lime.*—The following is an analysis made of an excellent hydraulic lime used in the construction of the great canal in the state of New York:

Carbonic acid	35.05
Lime	25.
Silex	15.05
Alumine	16.05
Water	5.03
Oxide of iron	2.02
	98.2

It will not slake, but is pulverized, and then mixed with half its quantity (probably bulk) of sand.

§ II. ELECTRICITY, MAGNETISM, &c.

1. *Effects of Lightning.*—A house at Geneva, which had plates of tinned iron disposed in various ways about the roof and other parts, was struck by lightning on the 3d of July. The lightning produced various effects here and there, but among them none was more remarkable than that produced on one of the plates of tin that had been placed on the roof against the chimney. This plate is pierced with two circular holes, about an inch in diameter each, and four inches apart. Each hole is strongly burred, but the remarkable circumstance is, that the burs of the two holes are in opposite directions, which, according to M. Pictet, either indicates that the electric fluid has passed through the plate forming one hole, has moved five inches along it, and then gone through again; or that there were two currents of fluid which moved simultaneously in opposite directions, at five inches distance from each other.

2. *Production of Magnetism.*—M. Poenitz of Dresden has lately experimented on the production of magnetism by hammering, friction, &c., and has come to the conclusion, per-

haps before drawn, that it is not produced in the iron, but given to it by the external magnetism of the earth ; all that the motion given to the iron in the various mechanical processes used, does, is to render it more susceptible of receiving magnetism. One of his modes of giving this motion to the particles, is to fix one end of a rod of iron or steel steadily, placing the rod in any position required, as either perpendicular or parallel to the dip, &c., and then to make it vibrate by drawing the free end from the axis of the rod, and suddenly letting it loose. If in a favourable position, the iron soon acquires magnetism.

3. *Effect of Iron on the rate of Chronometers.*—Mr. Barlow, of Woolwich, has lately made experiments on the effect of iron, free from any permanent magnetism, in altering the rates of chronometers placed in its vicinity. Mr. Fisher had remarked the difference of rate occurring in his chronometer when on board and on shore, though the vessel was frozen in, and therefore could produce no variation by its motion. He therefore attributed it to the magnetic action of the iron in the vessel, a conclusion that has been confirmed by Mr Barlow's experiments.

In these experiments, various chronometers were placed in different positions near a mass of unmagnetic iron, and an alteration in the rates of them observed. This alteration varied with their respective situations to the iron, but was always uniform in the same position. The effects amounted sometimes to 5″ per day, and were at last traced to magnetism in the balance and spring of the chronometer.

The plan suggested by Mr. Barlow, of estimating the effects of a ship's metal on the compass needle, namely, by placing a plate of iron in such a position, and so near to the needle, as to be equal in effect to the rest of the metal in the ship, is well known, and the return of his Majesty's ship Leven, from a voyage of sixteen months, affords proof, by the experiments that have been made on board, of its value. The same plan is now proposed by him for ascertaining the ship-rates of chronometers before they go on board, by simply taking their rates on a certain situation, and at a proper distance, from such a plate of iron.

4. *Electro-Magnetic Rotation.*—An ingenious little instrument has been invented by M. Ampere, in illustration of the rotatory motion of the wire round the pole of a magnet. Its advantage consists in comprising the voltaic combination used in itself. Suppose a cylinder of copper, about two inches in diameter and the same in height, and within it a smaller cylinder about half an inch in diameter; these are fastened together by a bottom, having a hole in its centre the size of the smaller cylinder, so

that the two cylinders form, as it were, a circular cell for acid: a piece of metal is fastened from side to side, like a bridge, across the top of the smaller cylinder, and from the middle of it rises a piece of wire, supporting at its top a small metal cup, containing mercury. A short cylinder of zinc is then procured, of a size that will permit it to go freely into the copper cell before described: a wire, in the form like the letter U inverted (∩), is soldered to it at opposite sides, and in the bend of this wire a metallic point is fixed, which, when placed in the little cup of mercury before described, suspends the zinc cylinder freely in the copper cell; then weak acid being put into the cell, the zinc and copper form a voltaic combination, and the two sides of the ∩ wire are both in the same state, so that the pole of a small magnet placed in the cylinder, that is left open in the axis of the apparatus, makes the wire, and the zinc cylinder with it, revolve. If the apparatus be 9 or 10 inches in diameter, it is stated that there is a tendency to rotation by the action of the terrestrial magnetism alone.

5. *Note on New Electro-Magnetical Motions, by* M. Faraday, —At page 96 of this volume, I mentioned the expectation I entertained of making a wire through which a current of voltaic electricity was passing, obey the magnetic poles of the earth in the way it does the poles of a bar magnet. In the latter case it rotates, in the former I expected it would vary in weight; but the attempts I then made, to prove the existence of this action, failed. Since then I have been more successful, and the object of the present note is so far to complete that paper, as to shew in what manner the rotative force of the wire round the terrestrial magnetic pole, is exerted, and what the effects produced by it, are.

Considering the magnetic pole as a mere centre of action, the existence and position of which may be determined by well-known means, it was shewn by many experiments, in the paper, page 74, that the electro-magnetic wire would rotate round the pole, without any reference to the position of the axis joining it with the opposite pole in the same bar; for sometimes the axis was horizontal, at other times vertical, whilst the rotation continued the same. It was also shewn that the wire, when influenced by the pole, moved laterally, its parts describing circles in planes perpendicular nearly to the wire itself. Hence the wire, when strait and confined to one point above, described a cone in its revolution, but when bent into a crank, it described a cylinder; and the effect was evidently in all cases for each point of the wire to describe a circle round the pole, in a plane perpendicular to the current of electricity through the wire. In dispensing with the magnet, used to give these motions, and operating with the terrestrial magnetic pole, it was easy, by applying the information gained above, to deduce beforehand the direction the motions would probably take: for, assuming that the dipping-needle, if it does not point to the pole of the earth, points at least in the direction in which that pole is active, it is evident that a straight electro-magnetic wire, affected by the terrestrial as by an artificial pole, would move laterally at right angles to the needle; that is to say, it would endeavour to describe a cylinder round the pole, the radius of which may be represented by the line of the needle prolonged to the pole itself. As these cylinders, or circles, would be of immense magnitude, it was evident that only a very minute portion of them could be brought within the reach of experiment; still, however, that portion would be sufficient to indicate their existence, inasmuch as the motions taking place in the part under consideration, must be of the same kind, and in the same direction, as in every other part.

Reasoning thus, I presumed that an electro-magnetic wire should move laterally, or in a line perpendicular to the current of electricity passing through it, in a plane perpendicular to the dipping-needle; and the dip being here 72° 30′, that plane would form an angle with the horizon of 17° 30′, measured on the magnetic meridian. This is not so far removed from the horizontal plane, but that I expected to get motions in the latter, and succeeded in the following manner:—A piece of copper wire, about .045 of an inch thick, and fourteen inches long, had an inch at each extremity bent at right angles, in the same direction, and the ends amalgamated; the wire was then suspended horizontally, by a long silk thread, from the ceiling. A basin of clean pure mercury was placed under each extremity of the wire, and raised till the ends just dipped into the metal. The mercury in both basins was covered by a stratum of diluted pure nitric acid, which dissolving any film, allowed free motion. Then connecting the mercury in one basin with one pole of Hare's Calorimotor, the instrument mentioned page 74, the moment the other pole was connected with the other basin, the suspended wire moved laterally across the basins till it touched the sides: on breaking the connexion, the wire resumed its first position; on restoring it, the motion was again produced. On changing the position of the wire, the effect still took place; and the direction of the motion was always the same relative to the wire, or rather to the current passing through it, being at right angles to it. Thus when the wire was E and W, the E end to the zinc, the W end to the copper plate, the motion was towards the north; when the connexions were reversed, the motion was towards the south. When the wire hung N and S, the N end to the zinc plate, the S end to the copper plate, the motion was towards the W; when the connexions were reversed, towards the E; and the intermediate positions had their motions in intermediate directions.

The tendency, therefore, of the wire to revolve in a circle round the pole of the earth, is evident, and the direction of the motion is precisely the same as that pointed out in the former experiments. The experiment also points out the power which causes Ampere's curve to traverse, and the way in which that power is exerted. The well-known experiment, made by M. Ampere, proves, that a wire ring, made to conduct a current of electricity, if it be allowed to turn on a vertical axis, moves into a plane east and west of the magnetic meridian; if on an E and W horizontal axis, it moves into a plane perpendicular to the dipping-needle. Now if the curve be considered as a polygon of an infinite number of sides, and each of these sides be compared in succession to the straight wire just described, it will be seen that the motions given to them by the terrestrial pole, or poles, are such as would necessarily bring the polygon they form into a plane perpendicular to the dipping-needle; so that the traversing of the ring may be reduced to the simple rotation of the wire round a pole. It is true the whole magnetism of the earth is concerned in producing the effect, and not merely that portion which I have, for the moment, supposed to respect the north pole of the earth as its centre of action; but the effect is the same, and produced in the same manner; and the introduction of the influence of the southern hemisphere, only renders the result analogous to the experiment at page 82, where two poles are concerned, instead of that at page 77, &c., where one pole only is active.

Besides the above proof of rotation round the terrestrial pole, I have made an experiment still more striking. As in the experiment of rotation round the pole of a magnet, the pole is perpendicular to but a small portion of the wire, and more or less oblique to the rest, I considered it probable, that a wire, very delicately hung, and connected, might be made to rotate round the dip of the needle by the earth's magnetism alone; the upper part being restrained to a point in the line of the dip, the lower being made to move in a circle surrounding it. This result was obtained in the following manner: a piece of copper-wire, about 0.018 of an inch in diameter, and six inches long, was well amalgamated all over, and hung by a loop to another piece of the same wire, as described at page 285, so as to allow very free motion, and its lower end was thrust through a small piece of cork, to make it buoyant on mercury; the upper piece was connected with a thick wire, that went away to one pole of the voltaic apparatus; a glass basin, ten inches in diameter, was filled with pure clear mercury, and a little dilute acid put on its surface as before; the thick wire was then hung over the centre of the glass basin, and depressed so low that the thin moveable wire having its lower end resting on the surface of the mercury, made an angle of about 40° with the horizon. Immediately the circuit through the mercury was completed, this wire began to move and rotate, and continued to describe a cone whilst the connexions were preserved, which, though its axis was perpendicular, evidently, from the varying rapidity of its motion, regarded a line parallel to the dipping needle as that in which the power acted that formed it. The direction of the motion was, as expected, the same as that given by the pole of a magnet pointing to the south. If the centre from which the wire hung was elevated until the inclination of the wire was equal to that of the dip, no motion took place when the wire was parallel to the dip; if the wire was not so much inclined as the dip, the motion in one part of the circle capable of being described by the lower end was reversed; results that necessarily follow from the relation of the dip and the moving wire, and which may easily be extended.

I have described the effects above as produced by the north pole of the earth, assuming that pole as a centre of action, acting in a line represented by the dip of the needle. This has been done that the phenomena might more readily be compared with those produced by the pole of a magnet. M. Biot has shewn by calculation that the magnetic poles of the earth may be considered as two points in the magnetic axis very near to each other in the centre of the globe. M. Ampere has in his theory advanced the opinion that the magnetism of the earth is caused by electric currents moving round its axis parallel to the equator. Of the consonance existing among the calculation, the theory and the facts, some idea may perhaps be gained from what was said, page 86, on the rotation of a pole through and round a wire ring. The different sides of the plane which pass through the ring, there described, and which may represent the equator in M. Ampere's theory, accord perfectly with the hemispheres of the globe; and the relative position of the supposed points of attraction and repulsion, coincide with those assigned by M. Biot for the poles of the earth itself. Whatever, however, may be the state and arrangement of terrestrial magnetism, the experiments I have described bear me out, I think, in presuming, that in every part of the terrestrial globe an electro-magnetic wire, if left to the free action of terrestrial magnetism, will move in a plane (for so the small part we can experiment on may be considered), perpendicular to the dip of the needle, and in a direction perpendicular to the current of electricity passing through it.

Reverting now to the expectation I entertained of altering the apparent weight of a wire, it was founded on the idea that the wire, moving towards the north round the pole, must rise, and moving towards the south must descend; inasmuch as a plane perpendicular to the dipping needle, ascends and descends in these directions. In order to ascertain the existence of this effect, I bent a wire twice at right angles, as in the first experiment described in this note, and fastened on to each extremity a short piece of thin copper wire amalgamated, and made the connexion into the basins of mercury by these thin wires. The wire was then suspended, not as before, from the ceiling, but from a small and delicate lever, which would indicate any apparent alteration in the weight of the wire; the connexions were then made with the voltaic instrument, but I was surprised to find that the wire seemed to become lighter in both directions, though not so much when its motion was towards the south as towards the north. On farther trial it was found to ascend on the contacts being made, whatever its position to the magnetic meridian, and I soon ascertained that it did not depend on the earth's magnetism, nor on any local magnetic action of the conductors, or surrounding bodies, on the wire.

After some examination I discovered the cause of this unexpected phenomenon. An amalgamated piece of the thin copper-wire was dipped into clean mercury, having a stratum of water or dilute acid over it; this, however, was not necessary, but it preserved the mercury clean and the wire cool. In this position the cohesive attraction of the mercury raised a little elevation of the metal round the wire of a certain magnitude, which tended to depress the wire by adding to its weight. When the mercury and the wire were connected with the poles of the voltaic apparatus, this elevation visibly diminished in magnitude by an apparent alteration in the cohesive attraction of the mercury, and a part of the force which before tended to depress the wire was thus removed. This alteration took place equally, whatever the direction in which the current was passing through the wire and the mercury, and the effect ceased the moment the connexions were broken.

Thus the cause which made the wire ascend in the former case was evident, and by knowing it, it was easy to construct an apparatus in which the ascent should be very considerable. A piece of copper bell-wire, about two inches long, had portions of the amalgamated fine copper-wire soldered on to its ends, and those bent downwards till parallel to each other. This was then hung by a silk thread from the lever, and the fine wire ends dipped into two cups of clean mercury. When the communications were completed from the voltaic instrument through these two cups, the wires would rise nearly an inch out of the mercury, and descend again on breaking the communition.

Thus it appears that, when a fine amalgamated copper-wire dips into mercury, and a current of voltaic electricity passes through the combination, a peculiar effect is produced at the place where the wire first touches the mercury, equivalent to a diminution of the cohesive attraction of the mercury. The effect rapidly diminished by increasing the size of the wire, and 20 pair of plates of Dr. Wollaston's construction, and four inches square, would not produce it with the fine wire: on the contrary, two large plates are sufficient. Dr. Hare's calorimotor was the instrument used, and the charge was so weak that it would barely warm two inches of any sized wire. Whether the effect is an actual diminution of the attraction of the particles of the mercury, or depends on some other cause, remains as yet to be determined. But in any case its influence is so powerful, that it must always be estimated in experiments made to determine the force and direction of an electro-magnetic wire, acted on by a magnetic pole, if the direction is otherwise than horizontal, and if they are observed in the way described in this note. Thus, at the magnetic equator, for instance, where the apparent alteration of weight in an electro-magnetic wire may be expected to be greatest, the diminution of weight in its attempt to ascend would be increased by this effect, and the apparently increased gravity produced by its attempt to descend would be diminished, or perhaps entirely counteracted.

I have received an account by letter from Paris, of an ingenious apparatus (see page 415,) contrived by M. Ampere, to illustrate the rotatory motions described in my former paper. M. Ampere states that, if made of sufficient size, it will rotate by the magnetic action of the earth, and it is evident that that will be the case in latitudes at some distance from the equator, if the rotatory wires, namely, those by which the ring of zinc is suspended, are in such a position as to form an angle with a vertical line, larger than that formed by the direction of the dip.

It is to be remarked, that the motions mentioned in this note were produced by a single pair of plates, and therefore, as well as those described in the paper, page 74, are the reverse of what would be produced by two or more pair of plates. It should be remembered also, that the north pole of the earth is opposite in its powers to what I have called the north poles of needles or magnets, and similar to their south poles.

I may be allowed, in conclusion, to express a hope that the law I have ventured to announce, respecting the directions of the rotatory motions of an electro-magnetic wire, influenced by terrestrial magnetism, will be put to the test in different latitudes; or, what is nearly the same thing, that the law laid down by M. Ampere, as regulating the position taken by his curve, namely, that it moves into a plane perpendicular to the dipping-needle, will be experimentally ascertained by all those having the opportunity.

III. NATURAL HISTORY.

§ I. MINERALOGY, METEOROLOGY, &c.

1. *Oxalate of Iron found as a Mineral.*—This curious substance was found by M. Breithaupt, in friable lignite, and at first supposed to be eisen resin, or mellate of iron; but M. M. de Rivero, on analyzing it, found it to be a compound of oxalic acid and protoxide of iron. It was found in small flat crystalline pieces of a yellow colour, and similar in appearance to the protoxalate of the laboratory. It was scratched by the nail. Its specific gravity was 1.3; and when rubbed, it became negatively electrical. All its properties agreed with those of oxalate of iron, and it was distinguished from mellate of iron in the circumstance that when decomposed by ammonia, the ammoniacal solution did not precipitate alum, an effect which does take place with the mellate. When decomposed by ammonia, protoxide of iron and oxalate of ammonia were obtained, and the elements appeared to be combined in the proportion of

53.86	protoxide of iron
46.14	oxalic acid.

The name proposed for this mineral is Humboldtine.—*Ann. de Chim.*, xviii., p. 207.

2. *Native Copper of Lake Superior.*—Silliman's *Journal of Science*, Vol. III., contains an account, by Mr. Schoolcraft, of the native copper mines on the southern shore of Lake Superior. The first appearances of the copper are found on the head of the portage across Keweena-point, 270 miles beyond the Soult de St. Mara, and continues from thence to the river Ontonagon. The copper is metallic, and occurs in the pebbles disseminated in particles from the size of a grain of sand to lumps of two pounds in weight. A strip of alluvial sand extends from the lake up the river two or three leagues, to which succeed high broken hills of a sterile aspect, among which are the copper mines, at a distance of 32 miles from the lake. One very large mass of native copper reposes on the west bank of the river, at the foot of a bank of alluvion, lying amidst blocks of granite, hornblende, and other bodies peculiar to the soil of that place. The copper is pure and malleable, very bright, and lies in connection with serpentine rock, the face of which it nearly overlays, and is also disseminated through the substance of the stone. The rock is of an irregular form, abcut 3ft. 8in. long, and 3ft. 4in. wide; it may contain altogether about 11 cubical feet. The weight of metallic copper in the rock is estimated at 2200lb., but the quantity has evidently been much diminished since its first discovery.

Mr. Schoolcraft adds, that the serpentine rock is not in situ, nor is it so found in any part of the region he visited. He thinks that the source of this copper, and that found in the region of the Ontonagon, is the Porcupine Mountains. These are situated 30 miles westward of the river, and are supposed to be the seat of extinguished volcanoes, that have thrown forth these masses of native copper. This opinion is supported by the fact of the mountains being composed (as far as is observed) of granite, probably associated with other primary rocks, and among them serpentine; that the red sandstone-rock at their base is highly inclined towards the mountains, so as to be almost vertical, being apparently thrown up by the granite; and also by the height (1800 feet above the lake) of the peaks, their conical and rugged forms, and other appearances of volcanic mountains.

Mr. Schoolcraft then states the occurrence of native copper in islands on the other lakes and localities, but forming part of the same region. He concludes by stating it as his opinion, that though the alluvial soil along the banks of the Ontonagon up to its source, and in the contiguous region, contains very frequent, and some most extraordinary imbedded masses of copper, yet there is no body of it sufficiently extensive to become the object of mining operations; but he thinks that a mineralogical survey of the rock formation skirting the Ontonogon, would lead to the discovery of very valuable mines of the sulphuret and carbonate of copper, the working of which would be rendered still more advantageous by the occasional occurrence of masses of native copper.—*American Journal,* iii. 201.

3. *Mont Brasier.*—There is a mountain called Mont Brasier, situated between Senes and Larógne, in the Alps, which has been reported from time to time to emit noises and flame. This report has been verified by M. Dubois-Aymé, who, when in the neighbourhood in 1818, examined the mountain, collected evidence of the flames having been seen, and himself heard detonation in the mountain. On examining the mountain, he found that in the strata of limestone that formed its sides were beds of pyritous chalk, marly schists, radiated sulphuret of iron, bituminous substances, &c., and he concludes that the flames and noises are produced by the accidental firing of hydrogen liberated by the action of water on these substances. It is to be remarked that the detonations are most frequent when the wind is in one direction, an effect probably due to the mixture that then takes place of air with the hydrogen within the crevices of the mountain.—*Ann. de Chim.*, xviii., p. 158.

4. Scipione Mazella's *Account of the Rise of Monte Nuovo, in the Year* 1538.

[To the EDITOR of the *Quarterly Journal of Science.*]

DEAR SIR—I send you an account of the Monte Nuovo, extracted from a Description of the Environs of Naples, by Scipione Mazella, printed there in the year 1594, fifty-six years after the formation of the mountain: as the Latin description is quoted from an earlier work, it is not improbable that the author may have been a contemporary, if not an eye-witness of the remarkable event he commemorates.

Believe me very sincerely yours,
ASHHURST MAJENDIE.

" Opposite to the Monte Barbaro is a mountain about three miles in circumference, and nearly of the same height as the Monte Barbaro: it extends on the south towards the sea, on the north to the Lake Avernus, on the west nearly to the Sudatorio, and on the east joins the base of the Monte Barbaro: it is called by the country-people Monte Nuovo, because it was formed in the space of a day and a night. In the year 1538, on the 29th of September, several earthquakes having been felt throughout the whole district of Puzzuolo on the preceding days, the earth opened near Tripergola with a terrible sound like thunder, so that it was expected that the whole country would have been destroyed: the sky was then serene, and from the aperture burst forth flames of fire, bearing with them cinders and red hot stones, with dense smoke: these stones were carried up into the air with such force, that it was a wonderful and terrible thing to contemplate. Afterwards the wind rose with great fury, and the cinders were dispersed on all sides, and driven even into Africa. So vast a quantity of stones and cinders were thrown round the chasm, as to form the mountain now called the Monte Nuovo. In consequence of this chasm, and the formation of the mountain, the castle of Tripergola, with a great part of the lakes Avernus and Lucrine, and all those antient and noble buildings which were on their banks, and most of the baths were overwhelmed. The celebrated philosopher Simone Portio of Naples wrote a learned treatise on the burning of Tripergola, and relative to our subject states as follows: " Puteolorum regio fuit biennio fere magnis terremotibus agitata, ut nulla in ea superesset domu integra, quæ proximam minabatur ruinam. At die vigessimo septimo et vigessimo octavo Septemb. anno 1538 perpetuis diebus, et noctibus terra est commota; mare passibus fere 200 recessit quo loco Accolæ ingentem piscium multitudinem cæperunt et aquæ dulces erant. Die vero 29 magnus terræ tractus, qui inter radices montis Gauri (quem Barbarum appellant), et mare juxta Avernum jacet, sese erigere videbatur, et montis subito nascentes imitari figuram. Eo ipso die horâ noctis 2 hic terræ cumulus aperto veluti ore, magno cum fremitu miros evomuit ignes, pumices, lapides, cineresque fœdi tam magnam copiam, ut quæ adhuc extabant Puteolorum ædificia operuerit, herbas omnes texeret, arbores fregeret, pendentemque vindemiam ad sextum usque lapidem in cineres verterit, aves, et nonnullas quadrupedes bestias interemerit. Fugientibus per tenebras Puteolanis cum natis, et uxoribus magno ejulatu Neapolem sese recipientibus, cinis propè voragineus erat siccus, longè vero lutesus, et humidus cecedit. Sed quod omnem superat admirationem, mons circum eam voraginem (nunc dicitur mons novus) ex pumicibus, et cinere plusquam 1000 passuum altitudine una nocte congestus aspicitur, in quo multa in erant spiramenta, e quibus nunc duo supersunt, alterum juxta Avernum, alterum in medio montis ; Averni magna pars operta cinere. Balnea ita tot seculis celebrata, quæque tot ægris salutem præstabant cinere sepulta jacent, durat hoc incendium usq ; ad hanc diem cum aliqua tamen intercapedine."

5. *Fall of an Aërolite at Juvinas.*—A large aërolite fell on the 15th June last at Juvinas, a village in the arrondissement of l'Argentière, department de l'Ardèche, respecting which some very accurate details have been preserved. It fell about four o'clock P.M., the sky being clear, and the sun shining bright; a continued rolling noise was heard for above three minutes, during which time four distinct detonations took place. The noise was heard at Tarascon, at Nismes, and still farther off. A brilliant fire was seen in the air by persons, at Nismes, St. Thome, (a league to the west of Viviers), and Aps, a league farther off; all agree in saying it resembled a fire, burning like a star, and descending slowly in the N.W., and which, on disappearing, left a train of smoke. At first strange reports were circulated about the fire and the noise, but after eight or nine days, two countrymen of Juvinas, a village 20,600 toises to the N.W. of Viviers, described, that whilst working on the land they heard a dreadful noise, and saw an enormous mass of fire fall about fifty steps off from them into the ground ; tearing up the surface, and making much smoke. Being frightened they ran away, and at first durst not mention the circumstance ; but after a time, other persons becoming acquainted with the fact, search was made in the ground where the fire descended ; and, at the depth of five feet a large stone was discovered, weighing 220lb., or 91 kilogrammes. The countrymen, now relieved from their fright, thought from its weight that it contained gold, and could not be dissuaded from breaking it up. Large portions of it were, however, preserved. Some of the fragments are with Dr. Embri of Aubenas, others with M. Allijas, watch-maker

of the same place, and some which had been seen by M. Flaugergues, by whom the account is drawn up, belonged to Dr. Mausin. The stone appeared to be composed of two substances. The one in largest proportion is grey, and mixed with small black-shining grains. The surface is covered by a thin stratum, resembling a black and polished glare, not unlike that of the potteries. It appears to be formed of the black particle before spoken of, that have been fused at the surface, and has been very fluid, for the smallest inequalities are cased by it without being filled up. The stone is moderately hard, is scratched by steel instruments, does not strike fire with steel, and is not acted on by nitric acid.

M. Flaugergues agrees with those theorists who would consider this stone as having formed a very small comet or planetary body which has been met in its course by the earth.—*Journ. de Phys.* xcii. p. 463.

In a further account of this aërolite given by M. L. A. D. Firman, it is stated that another meteoric stone a kilogramme in weight, was found a little distance off, and several small ones at Mayras, near to Juvinas. M. de Malbos, who happened to be at Barias when the stone fell, was looking towards the place where it first appeared at the time. He saw a globe of fire descend perpendicularly from the heavens. He shewed it to his workmen, and counting his pulse estimated the time between its appearance and the explosion that followed, at five seconds. He observed also the obscure vapoury trace left by the meteorolite in the air. It was not continued to the earth but ceased to be emitted before the stone reached the ground, and remained seven or eight minutes undissipated.—*Journ. de Phys.* xciii, p. 71.

6. *Earthquake in Cunnemara.*—Upwards of one hundred acres of the lands of Letterbrocken, part of the property of the Provost of Trinity College in Joyce County, and consisting of prime pasture and mountain, on which a number of tenants resided, commenced moving and carrying with it huge rocks, immense masses of earth, the entire crop of wheat, oats, potatoes, &c., precipitated the whole into the sea and disappeared. Previous to its movement, a great noise was heard for some time, and the ground was convulsed. It is supposed that the previous drought which had occurred, prepared the way for this phenomenon. Two days after, a large tract of land thickly inhabited, the property of R. Martin, Esq. M. P., in the same neighbourhood, was visited with a like phenomenon, but even of a more destructive nature; the loss of the sufferers not being confined to their land and crops, but their entire stock and property being swallowed up by the earthquake. These occurrences are mentioned in the *Gents. Mag.* for November, from the *Tuam Gazette*, and their date given as ten days previous.

7. *Earthquake at Celebes.*—The Batavian Journals of April give an account of an earthquake very destructive in its effects which took place on the 29th of December 1820, on the south coast of Celebes. It did immense damage, particularly at Boelækomba where the sea rose several times a prodigious height, and then falling again with great rapidity, alternately deluged and left the shores, destroying all the plantations from Bontain to Boelækomba. Many hundred persons lost their lives. The forts of Boelækomba and Bontain were much damaged.

On the 4th of January this year, another shock of an earthquake occurred in the same neighbourhood.

8. *Mud Volcanoes in Bogs.*—A small tract of bog, called Forest Bog, about one mile and a half from Mount-mellick, situate about sixteen miles in a north-east direction from the Bog of Kilmaleady, has been strangely agitated for some days. It boils upwards, rising to a considerable height, the matter thrown up falling again into the basin from whence it issued. It has not overflown; but the people of the neighbourhood are in dread of some catastrophe occurring. The phenomena resemble those of the mud volcanoes of America, and are probably occasioned by internal fermentation.

9. *Scottish Pearl.*—A very fine pearl was found not long since in the river Tay, and is supposed to be one of the finest ever found in Scotland. It is hardly to be surpassed either for size or beauty by any oriental pearl. It is in the possession of Mr. C. Murray, Jeweller, in Perth.

10. *Rein Deer.*—Mr. Bullock has succeeded in bringing specimens of the Rein Deer to this country, and hopes are entertained that they may lead to the colonization of our mountain-forests by this animal. While on a tour in Norway he procured a herd of twenty, which were destroyed by eating a poisonous plant which grew on a small island on which they were kept. He then bought a second herd of twelve, and succeeded in bringing them alive and well into the Thames. Here however, in consequence of the custom-house officer not feeling authorized to allow the deer to be landed, eight died on board the vessel before permission could be obtained from the authorities in London. The remnant saved consists of a male and female, a fawn (since dead), and a male which has been cut. The latter is about ten hands high and proportionably stout. The others are a hand or two lower. Their fur is very thick and fine, and delicately warm and soft. Their horns branch beautifully and are covered with a short fur. The antlers of the largest animal are three feet in length. Their hoofs are very broad and flexible between the divisions, enabling them to clamber up pre-

cipices and hang on rocks inaccessible to other animals. They are very swift. They seem reconciled to hay as food, and like brandy which is administered to them as a medicine.

With the deer Mr. Bullock has brought a native Laplander, his wife and child. These beings are about four feet eight inches in height; the man being of the common size, the woman rather tall. The child is about five years old.—*New Mon. Mag.* p. 506.

11. *First appearance of the Boa Constrictor in the Island of St. Vincent.*—A most singular circumstance occurred last week in the Charaib country, when some negroes, who were working near Sandy Bay, discovered an immense serpent, hitherto wholly unknown in any of these islands, and which was shot through the head by one of the party. It is supposed to be a species of *Boa* so common on the neighbouring continent, but in what way it reached the shores of St. Vincent is quite unknown. Its entire length was between fourteen and fifteen feet, the circumference of the body between three and four feet. When first seen it was lying in a coil, but raised itself on being roused.—*Royal Gazette and Bahama Advertiser*, August 1821.

§ II. Medicine.

1. *Salivation and Ulceration of the Gums produced by Hydrocyanic or Prussic Acid.*—Dr. Macleod, in his experience with the prussic acid used as a medicine, has had occasion to notice three instances in which the administration of it produced ulceration of the gums with salivation. In the first instance, the effect was slight; in the second, it ceased with the discontinuance of the medicine, and returned when that was re-administered; in the third, the ulceration was very severe, extensive, and difficult to heal, and the soreness of the mouth exceedingly distressing; the medicine was discontinued, but on being repeated about three months afterwards, the effects were reproduced.

2. *Preservative against Scarlet Fever.*—It is announced in the *Journal de Medicina Pratique* of Berlin, that the Belladonna is a preservative against this fever. The fact was first discovered at Leipsig, but it has lately been confirmed by several experiments.

3. *On Irritation of the Spinal Nerves.*—

[To the Editor of the *Quarterly Journal of Science.*]

Sir—I take the liberty to submit to your notice a pathological fact, which has not, to the best of my knowledge, been generally remarked, and attention to which, as far as my own experience goes, promises some diminution of those difficulties with which the healing art has to contend. Most medical practitioners who have attended to the subject of spinal disease, must have observed that its symptoms frequently resemble various and dissimilar maladies, and that commonly the function of every organ is impaired whose nerves originate near the seat of the disorder. The occurrence of pain in *distant parts* forcibly attracted my observation, and induced frequent examinations of the spinal column; and after some years' attention, I consider myself enabled to state, that in a great number of diseases, morbid symptoms may be discovered about the origins of the nerves which proceed to the affected parts, or of those spinal branches which unite with them, and that if the spine be examined, more or less pain will commonly be felt by the patient on the application of pressure about or between those vertebræ from which such nerves emerge. If disease is confined to one side of the body, or one arm or one leg, this tenderness will be felt on the *same side* of the spine *only*; but if central parts, or both sides of the body, or both arms or legs, are diseased, tenderness will be felt on *both sides* of the spine. This symptom has been found to attend various other affections. This spinal affection may perhaps be considered as the *consequence* of diseases, but of its existence at their commencement any person may satisfy himself; and this circumstance, combined with the success which has attended the employment of topical applications to the tender parts about the vertebræ, appear to indicate that the *cause may* exist there. Prejudice sometimes operates against the idea of connexions so remote; but in many instances patients are surprised at the discovery of tenderness in a part, of whose implication in disease they had not the least suspicion.

The opinion entertained by some of our Continental neighbours, of the importance of the spinal brain in disease, is well known. That many of our maladies are the sympathetic consequences of the operation of a distant cause, and that diseases apparently the most dissimilar may have one common origin, have been the doctrines of some of our own most eminent pathologists; and though the injuries inflicted on our frames by mechanical and chemical agencies usually manifest their effects at the part where the cause has acted, we may be *too* much disposed to generalize from these premises, and to conclude that the *cause* of pain, inflammation, and the other phenomena of disease, also exist *at the part* where the symptoms are perceived. The records, however, of disease abound with cases, which, combined with daily observation, and the discoveries of morbid anatomy, tend to shake our confidence in this very natural and general inference, and to indicate that the cause can, and frequently does, exist very remotely from its phenomena, and that vascular fulness about the origins of nerves can produce the most formidable symptoms in the parts to which they are distributed. Such instances I need not enumerate, but may be permitted to remark that they appear to corroborate the opinion, that all the degrees of morbid sensation, from the slightest tingling and itching to the severest pain, may be produced by one common cause, obstructed nervous function, of which the ultimate effects are spasm, convulsion, and paralysis.

I am, Sir,
Your very obedient servant,
Malmesbury, Dec. 10, 1821. RICHARD P. PLAYER.

4. *Medical Prize Question.*—The Academical Society of the Lower Loire has proposed a prize, consisting of a gold medal value 300 francs, for the best answer to questions respecting the yellow fever. It is required to trace its origin, to specify its causes and nature; to describe the state of the atmosphere and local circumstances where it prevails; to notify its identity or otherwise with similar fevers in Europe, *&c.*; to distinguish whether it be complicated with any other malady. There is also a second subject relating to the means for preventing its spreading, the proper modes of quarantine, *&c.* The memoirs to be sent, post free, to the Secretary of the Society before the 1st of May, 1822. Each to bear a motto with a repetition in a sealed paper, containing, as usual, the author's name and address.

5. *Medical Prize Question.*—The Royal Society of Medicine at Marseilles have proposed the following questions: 1. To determine the structure and functions of the spinal marrow. 2. To describe the nature, causes, symptoms, and treatment of the diseases by which the spinal marrow is affected. It is desired that clinical observations and pathological anatomy should be made the principal objects of the memoires. They may be written in Latin or French. The extent of time allowed is till July, 1822, and the prize a gold medal.

IV. GENERAL LITERATURE.

1. *Cabinet of Ancient Glass.*—A cabinet has been opened in Naples in the Studii Palace for the antique glasses found in Pompeii and Herculaneum. The collection contains a great variety of forms and colours, and proves that the ancients made use of glass as the moderns do, both in decorating their rooms, and in instruments of chemistry. The cabinet also contains a number of cinenary urns, for the most part enclosed in vases of lead.

2. *Lithography.*—A work is now in the course of publication at Stutgard, consisting of lithographic engravings by Strisner, from pictures of the early German masters, in the collection of Sulpice, Boisserée, and other amateurs. The whole when completed will contain 144 subjects; these are published in quarterly numbers in imperial folio, with descriptive, letter-press, written by Boisserée. The ability of the engraver which has been displayed in another lithographic work, that of the Munich Gallery, warrants the expectation of such a series of plates as will enable those hitherto unacquainted with the peculiar merits of the early painters of this school, fully to appreciate their excellence.

3. *Public Education.*—A plan has been lately suggested, and will be acted upon at Edinburgh, for instructing by lectures and demonstrations, the operative mechanics of that city, in the principles of those branches of science which are useful in the various trades that are carried on there. Lectures on practical mechanics and practical chemistry will be delivered twice a-week during the winter. A library has been formed, and the institution is conducted by a committee of fourteen, having a clerk and librarian. It is much to be desired that this plan were carried into execution in all the great towns of the United Kingdom, where it is possible: for efficacious as private interest has been found to be in improving the arts, yet there is no doubt that they must advance with the knowledge of the principles on which they are founded.

4. *Statistics of Spain.*—The Spanish monarchy is composed of Spain and her colonies. The kingdom of Spain contains 10,372,000 inhabitants spread over a surface of 24,661 square leagues. The colonies on a surface of 669,094 square leagues reckon 17,700,000 inhabitants distributed as follows;

	Square Leagues.	Inhabitants.
New Spain	118,477	7,550,000
Guatimala	43,089	1,200,000
Cuba and Florida . .	115,039	592,000
Puerto Rico & St. Domingo	2,805	493,000
New Grenada . . .	80,433	1,600,000
Caraccas	64,561	900,000
Peru	60,172	1,500,000
Chili	92,000	900,000
La Plata	144,955	1,100,000
The Philippines . . .	13,888	1,740,000
The Mariannes . . .	1,425	80,000

The revenues of Spain in 1817 amounted to 620,000,000 reals, (about 6,000,000*l.*)

In the kingdom of Denmark there were born in 1820, 32,376 children, among whom were 3,089 natural children. The number of deaths was 23,532. At Copenhagen the births were 2,225, including 852 natural children; the deaths 1,162.

5. *Census in America.*

	1800	1810	1820
Connecticut	251,002	261,942	273,248
New York	586,050	939,049	1,379,989
New Jersey	211,149	245,562	277,575
Pennsylvania	602,363	810,091	1,046,844
Delaware .	64,270	72,674	72,749
Maryland .	340,704	380,346	407,300

6. *Observatory at Abo in Finland.*—The emperor Alexander has erected a magnificent observatory at Abo, in Finland, the direction of which has been given to the celebrated astronomer Balbeck.

7. *Rewards for Discovery in the North.*—In the new Longitude Act, which is the 58th of Geo. III. amended, it is assumed that no ship has gone beyond 81° of N. lat., and 113° of W. long. within the arctic circle. The rewards proposed in it are,

£5,000 to any subject of Great Britain, who shall reach the longitude of 130°, from Greenwich, within the arctic circle.

£10,000, besides the above, for the North-West passage into the Pacific.

£1,000 for reaching 83° of North latitude; and a similar sum for 85°, 87°, and 89°, respectively.

8. *Convent of St. Bernard.*—A subscription has been entered into at Geneva, to be applied to the purpose of repairing and warming the convent on the Grand St. Bernard. It appears, that its inhabitants, who are known over the whole of Europe, as having saved many a wanderer from destruction, amidst the snows and precipices of their inclement mountain, suffer severely in their health, from the damp and cold which prevail in their crazy mansion; they have, therefore, the strongest possible claims upon the charitable traveller; and we trust that the British will not be backward in lending their aid towards the alleviation of their sufferings. The Helvetic Society has contributed the sum of 600 francs towards effecting the above objects.

Subscriptions are received by Messrs. Decandolle and Turrettini, bankers, at Geneva.

	MEAN PRESSURE OF ATMOSPHERE, *and Extremes.*				MEAN PRESSURE OF VAPOUR, *and Extremes.*				
	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarte
	ins.				ins.				grs.
Sept. Oct. Nov. 1820.	30.61 30.05 29.12				.635 .377 221				6.822 4.166 2.487
	30.27 29.63 28.74	30.61 29.84 28.74			.429 .284 .180	.635 .330 .180			4.770 3.206 2.061
Dec. January, February, 1821.	30.32 29.75 29.03				.415 .237 .120				4.616 2.701 1.391
	30.82 30.31 29.17	30.82 30.03 29.17	30.82 29.93 28.74		.375 .229 .134	.415 .233 .120	.635 .281 .120		4.195 2.619 1.524
March, April, May,	30.35 29.64 29.04				.429 .283 .186				4.750 3.190 2.166
	30.28 29.82 29.23	30.35 29.73 29.23			.490 .328 .174	.490 .305 .174			5.302 3.642 1.941
June, July, August.	30.30 29.99 29.59				.512 .375 .221				5.950 4.142 2.425
	30.28 29.90 29.50	30.30 29.94 29.50	30.35 29.83 29.23	30.82 29.88 28.74	.698 .490 .363	.698 .432 .221	.698 .368 .174	.698 .324 .120	7.366 5.340 3.922

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MEAN PRESSURE OF ATMOSPHERE, *and Extremes.*				MEAN PRESSURE OF VAPOUR, *and Extremes.*				MEAN WEIGHT OF VAPOUR, *and Extremes.*				MEAN DEGRE DRYNES *and Extrem*		
arter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Hal Yea
				ins.				grs.						
1 .05 12				.635 .377 221				6.822 4.166 2.487				20 5¼		
27 .63 74	30.61 29.84 28.74			.429 .284 .180	.635 .330 .180			4.770 3.206 2.081	6.822 3.686 2.081			9 1¾	20 3½	
32 9.75 .03				.415 .237 .120				4.616 2.701 1.391				19 2¼		
.82 0.31 .17	30.82 30.03 29.17	30.82 29.93 28.74		.375 .229 .134	.415 .233 .120	.635 .281 .120		4.195 2.619 1.524	4.616 2.660 1.391	6.822 3.173 1.391		20 2	20 2¼	20 3
.35 9.64 .04				.429 .283 .186				4.750 3.190 2.166				16 4½		
.28 9.82 .23	30.35 29.73 29.23			.490 .328 .174	.490 .305 .174			5.302 3.642 1.941	5.302 3.416 1.941			23 8	23 6¼	
.30 9.99 .59				.512 .375 .221				5.950 4.142 2.425				25 7		
.28 9.90 .50	30.30 29.94 29.50	30.35 29.83 29.23	30.82 29.88 28.74	.698 .490 .363	.698 .432 .221	.698 .368 .174	.698 .324 .120	7.366 5.340 3.922	7.366 4.741 2.425	7.366 4.078 1.391	7.366 3.625 1.391	20 5¾	25 6¼	2 6

TABLE I.

om *September* 1820, *to August* 1821.

EES OF SS, *mes.*		MEAN PRESSURE OF VAPOUR, *Corrected for 24 hours.*				MEAN DEGREES OF DRYNESS, *Corrected for 24 hours.*				MEAN EVAPORATION, *and Extremes.*				QUANTITY OF RAIN.		
alf ear	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Ha Yea
		ins.								grs.				ins.		
		.363				5½				1.48 .33				1.90		
		.273	.316			2¼	3¾			.59 .06	1.46 .19			1.57	3.47	
		.235				2				.65 .08				3.32		
20 3		.223	.229	.270		2¼	2¼	3		.55 .06	.65 .07	1.48 .13		.49	3.81	7.28
		.273				4¾				.78 .23				3.80		
		.322	.296			8	6¼			1.59 .53	1.59 .38			2.00	5.80	
		.365				7¼				1.96 .52				3.52		
25 6¼	25 4¾	.482	.422	.357	.313	6	6½	6½	4¾	1.73 .51	1.96 .52	1.96 .45	1.96 .29	3.04	6.56	12.3

TABLE I.

From September 1820, *to August* 1821.

MEAN WEIGHT OF VAPOUR, *and Extremes.*				MEAN DEGREES OF DRYNESS, *and Extremes.*				MEAN PRESSURE OF VAPOUR, *Corrected for 24 hours.*				MEAN DEGREES OF DRYNESS, *Corrected for 24 hours.*			
Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year
grs.								ins.							
6.822 4.166 2.487				20 5¼				.363				5½			
4.770 3.206 2.081	6.822 3.686 2.081			9 1¾	20 3½			.273	.316			2¼	3¾		
4.616 2.701 1.391				19 2¼				.235				2			
4.183 2.619 1.534	4.616 2.660 1.391	6.822 3.173 1.391		20 2	20 2¼	20 3		.223	.229	.270		2¼	2¼	3	
4.750 3.190 2.165				16 4½				.273				4¾			
5.302 3.642 1.941	5.302 3.416 1.941			23 8	23 6¼			.322	.296			8	6¼		
5.930 4.142 2.425				25 7				.365				7¼			
7.366 5.340 3.022	7.366 4.741 2.425	7.366 4.078 1.391	7.366 3.625 1.391	20 5¾	25 6¼	25 6¼	25 4¾	.482	.422	.357	.313	6	6½	6½	4¾

MEAN EVAPORATION, *and Extremes.*				QUANTITY OF RAIN.				MEAN TEMPERATURE, *and Extremes.*			
Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year
grs.				ins.							
1.48 .33				1.90				76 53¾ 62½ 45½ 35			
.59 .06	1.46 .19			1.57	3.47			57 43¼ 48 38¼ 31	76 48¼ 55 41¾ 31		
.65 .08				3.32				55 38½ 41¼ 36 24			
.55 .06	.65 .07	1.48 .13		.49	3.81	7.28		48 37¼ 41½ 33 25	55 38 41½ 34½ 24	76 43 48 38 24	
.78 .23				3.80				63 45½ 52¼ 38½ 30			
1.59 .53	1.59 .38			2.00	5.80			74 53 61¼ 44½ 35	74 49¼ 56¾ 41½ 30		
1.96 .52				3.52				78 56½ 65½ 47¼ 37			
1.73 .51	1.96 .52	1.96 .45	1.96 .29	3.04	6.56	12.36	19.64	82 63¼ 71¼ 54¾ 49	82 60 68½ 51 37	82 54½ 62¾ 46¼ 30	82 48¾ 55 42 24

To follow p. 114.

		MEAN PRESSURE OF ATMOSPHERE, *and Extremes.*				MEAN PRESSURE OF VAPOUR, *and Extremes.*				MEAN WEIGHT VAPOUR, *and Extreme*		
		Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Yea
		Ins.				Ins				Grs.		
1819	Autumn.	29.98				.429				4.697		
		30.01				.403				4.431		
1820		30.05				.377				4.166		
1819		29.73	29.87			.262	.355			2.986	3.944	
		29.68	29.85			.273	.342			3.097	3.815	
1820		29.63	29.84			.284	.330			3.206	3.686	
1819	Winter.	29.73				.219				2.505		
		29.74				.228				2.603		
1820		29.75				.237				2.701		
1820		29.79	29.76	29.81		.225	.222	.288		2.580	2.543	3.243
		30.05	29.89	29.87		.227	.227	.284		2.599	2.601	3.2
1821		30.31	30.03	29.93		.229	.233	.281		2.619	2,660	3.173
1820	Spring.	29.80				.268				3.013		
		29.72				.275				3.101		
1821		29.64				.283				3.190		
1820		29.95	29.88			.333	.300			3.680	3.346	
		29.88	29.80			.330	.302			3.661	3.381	
1821		29.82	29.73			.328	.305			3.642	3.416	
1820	Summer.	30.01				.421				4.610		
		30.00				.398				4.376		
1821		29.99				.375				4.142		
1820		29.91	29.96	29.92	29.87	.473	.448	.374	.331	5.143	4.888	4.11
		29.90	29.95	29.87	29.87	.481	.440	.371	.327	5.241	4.814	4.0
1821		29.90	29.94	29.83	29.88	.490	.432	.368	.324	5.340	4.741	4.07

TABLE II.

MEANS OF TWO YEARS.

HT OF , *es.*		MEAN DEGREES OF DRYNESS, *and Extremes.*				MEAN EVAPORATION, *and Extremes.*				MEAN QUANTITY OF RAIN.				T
alf ear	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Quarter	Year	Half Quarter
						Grs:				Ins.				
		6¾ 6 5¼				.50 .41 .33				2.36 2.13 1.90				56 55 53¾
		1¾ 1¾ 1¾	4½ 4 3½			.08 .07 .06	.32 .25 .19			3.47 2.52 1.57	5.83 4.65 3.47			38 40½ 43¼
		1 1½ 2¼				.03 .05 .08				2.02 2.67 3.32				32 35¼ 38½
43 208 73		1¾ 2 2	1½ 1¾ 2¼	3 3 3		.06 .06 .06	.05 .06 .07			1.18 0.83 0.49	3.20 3.50 3.81	9.06 8.17 7.28		34 35¼ 37¼
		5½ 5 4½				.27 .25 .23				1.63 2.71 3.80				43 44¼ 45½
		10½ 9¼ 8	8 7 6¼			.70 .61 .53	.48 .43 .38			1.51 1.75 2.00	3.14 4.47 5.80			54 53½ 53
		8 7½ 7				.59 .55 .52				2.39 2.95 3.52				59 57¾ 56½
117 .097 078	3.680 3.652 3.625	7¾ 6¾ 5¾	8 7 6¼	8 7 6¼	5½ 5 4¾	.66 .58 .51	.63 .57 .52	.55 .50 .45	.37 .33 .29	4.13 3.58 3.04	6.52 6.54 4.56	9.66 11.01 12.36	18.72 19.18 19.64	62½ 63 63½

TABLE II.

MEANS OF TWO YEARS.

	MEAN WEIGHT OF VAPOUR, *and Extremes.*			MEAN DEGREES OF DRYNESS, *and Extremes.*				MEAN EVAPORATION, *and Extremes.*				MEAN QUAN RAI	
	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter
								Grs:				Ins.	
1				6¾ 6 5¼				.50 .41 .33				2.36 2.13 1.90	
7	3.944 3.815 3.686			1¾ 1¾ 1¾	4½ 4 3½			.08 .07 .06	.32 .25 .19			3.47 2.52 1.57	5.83 4.65 3.47
03				1 1½ 2¼				.03 .05 .08				2.02 2.67 3.32	
99	2.543 2.601 2,660	3.243 3.208 3.173		1¾ 2 2	1½ 1¾ 2¼	3 3 3		.06 .06 .06	.05 .06 .07			1.18 0.83 0.49	3.20 3.50 3.81
01				5½ 5 4½				.27 .25 .23				1.63 2.71 3.80	
61	3.346 3.381 3.416			10½ 9¼ 8	8 7 6¼			.70 .61 .53	.48 .43 .38			1.51 1.75 2.00	3.14 4.47 5.80
76				8 7½ 7				.59 .55 .52				2.39 2.95 3.52	
41	4.888 4.814 4.741	4.117 4.097 4.078	3.680 3.652 3.625	7¾ 6¾ 5¾	8 7 6¼	8 7 6¼	5½ 5 4¾	.66 .58 .51	.63 .57 .52	.55 .50 .45	.37 .33 .29	4.13 3.58 3.04	6.52 6.54 4.56

ANTITY OF IN.		MEAN TEMPERATURE, *and Extremes.*				MEAN HIGHEST AND LOWEST.			
Half Quarter	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year
		56 55 53¾				63 50 62¾ 47¾ 62½ 45½			
		38 40½ 43¼	48 48 48¼			43 34 45½ 36 48 38¼	54 42 54½ 42 55 41¾		
		32 35¼ 38½				35½ 28½ 38¼ 32½ 41¼ 36			
9.06 8.17 7.28		34 35¼ 57¼	33 35½ 38	40¼ 42 43¼		38 32¾ 39¾ 33½ 41½ 33½	37 29 39 31½ 41½ 34½	45½ 35½ 47 36½ 48 38	
		43 44¼ 45½				48 38 50 38 52¼ 38½			
		54 53½ 53	48½ 49 49¼			63 45 62 44½ 61¼ 44	55 42 56 41¾ 56¾ 41½		
		59 57¾ 56½				68 50 66¾ 48½ 65½ 47¼			
9.66 11.01 12.36	18.72 19.18 19.64	62½ 63 63½	61 60½ 60	55½ 55 54½	48 48¼ 48¾	71 54 71 54½ 71¼ 54¾	70 52 69¼ 51½ 68½ 51	62 47 62¼ 46½ 62¾ 46	54 41 54½ 41½ 55 42

		MEAN.				HIGHEST IN SHADE.			
		Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year.	Year
Autumn,	1819	56				63			
						76			
						76			
	1820	53¾				62¼			
	1819	38	48			43	54		
						57	76		
						57	76		
	1820	43¼	48¼			48	55		
Winter,	1819	32				35½			
						53			
						55			
	1820	38½				41¼			
	1820	34	33	40		38	37	45½	
						50	53	76	
						48	55	76	
	1821	37¼	38	43		41½	41½	48¼	
Spring,	1820	43				48			
						66			
						63			
	1821	45½				52¼			
	1820	54	48½			63	55		
						77	77		
						74	74		
	1821	53	49¼			61¾	56¾		
Summer,	1820	59				68			
						89			
						78			
	1821	56½				65½			
	1820	62½	61	55½	48	71	70	62	54
						83	89	89	89
						82	82	82	82
	1821	63¼	60	54½	48¾	71¼	68½	62¾	55

TABLE III.

MEAN TEMPERATURE AND EXTREMES.

LOWEST IN SHADE.				LOWEST OF RADIATION.				HIGHEST IN SUN.				CONSTITUEN TURE OF	
alf arter	Qnarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter
0												54	
38												66.33	
35				26								66.35	
5½				39½								50¼	
4	42			30½								40	48¼
23	23			15								56.25	66.25
31	31			27	26							54.29	66.29
38¼	41¾			35¾	37½							42	46¼
28½				23¼								35	
11				5								53.10	
24				19				58				53.18	
36				33¾				42				37	
32¾	31	35		29	26							35½	35
19	11	11		12	5	5						49.22	53.10
25	21	24		19	19	19		80	80			50.21	53.18
33½	34½	38		29¾	31¾	34½		50¼	46½			36	36½
38				32¼								40½	
24				18								54.19	
30				22				110				54.30	
38½				31¼				71				42	
45	42			39¼	35¾							46½	43
33	24			24	18							58.27	58.19
35	30			25	22			114	114			58.28	58.28
44	41½			39¼	35½			88½	79¾			46	44
50				43¾								53½	
40				30								70.40	
37				31				130				61.35	
47¼				45½				97				50	
54	52	47	41	49	46½	41						57	55
42	40	11	11	35	30	18	5					67.46	70.40
49	37	21	24	39	31	22	19	144	144	144	144	69.49	69.35
54¾	51	46	42	51¼	48½	42	38	105	101	90¼		58	54

TABLE III.

MEAN TEMPERATURE AND EXTRE

	HIGHEST IN SHADE.				LOWEST IN SHADE.				LOWEST OF RADIATION.				
Year	Half Quarter	Quarter	Half Year.	Year	Half Quarter	Qnarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Ha Quar
	63				50								
	76				38								
	76				35				26				
	62¼				45½				39½				
	43	54			34	42			30½				
	57	76			23	23			15				
	57	76			31	31			27	26			
	48	55			38¼	41¾			35¾	37½			
	35½				28½				23¼				
	53				11				5				
	55				24				19				5
	41¼				36				33¾				42
	38	37	45½		32¾	31	35		29	26			
	50	53	76		19	11	11		12	5	5		
	48	55	76		25	24	24		19	19	19		8
	41½	41½	48¼		33½	34½	38		29¾	31¾	34½		50
	48				38				32¼				
	66				24				18				
	63				30				22				11
	52¼				38½				31¼				71
	63	55			45	42			39¼	35¾			
	77	77			33	24			24	18			
	74	74			35	30			25	22			1,4
	61¾	56¾			44	41½			39¼	35½			88
	68				50				43¾				
	89				40				30				
	78				37				31				13
	65½				47¼				45½				97
48	71	70	62	54	54	52	47	41	49	46½	41		
	63	89	89	89	42	40	11	11	35	30	18	5	
	82	82	82	82	49	37	21	24	39	31	22	19	14
48¾	71¼	68½	62¾	55	54¾	51	46	42	51¼	48½	42	38	105

EMES.

HIGHEST IN SUN.				CONSTITUENT TEMPERATURE OF VAPOUR.				CONSTITUENT TEMPERATURE OF VAPOUR, *Corrected for 24 hours.*			
alf arter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year	Half Quarter	Quarter	Half Year	Year
				54				53			
				66.33							
				66.35							
				50¼				49			
				40	48¼			38¼	46½		
				56.25	66.25						
				54.29	66.29						
				42	46¼			41	45		
				35				33¼			
				53.10							
58				53.18							
42				37				36¾			
				35½	35	42¼		34½	33½	40¼	
				49.22	53.10	66.10					
80	80			50.21	53.18	66.18					
50¼	46½			36	36½	41¾		35¼	36	40¾	
				40½				39¾			
				54.19							
110				54.30							
71				42				41			
				46½	43½			46	43		
				58.27	58.19						
114	114			58.28	58.28						
88½	79¾			46	44			45½	43¼		
				53½				52½			
				70.40							
130				61.35							
97				50				49¼			
				57	55½	50	46½	56½	54½	49½	44¾
				67.46	70.10	70.40	70.10				
144	144	144	144	69.49	69.35	69.35	69.18				
05	101	90¼		58	54¼	49½	45¾	57½	53½	48½	44¾

To follow p. **114.**

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LONDON:
PRINTED BY WILLIAM CLOWES,
Northumberland-court.